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AN EXPERIMENTAL INQUIRY INTO THE EXISTENCE AND NATURE OF 'INSIGHT'

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It was inevitable that the recent occupation with 'learning' should presently lead backward toward the consideration of a rapid, effective and 'intuitional' form of operation. 'Backward' because that had long been a favorite conception of learning, but a conception displaced by the simpler and more manageable forms of the reflex, automatic, psychomotor and ideational varieties which set the experiments of a generation ago. At that time the scientific standards coming into psychology placed a high value upon simple and mechanical concepts and procedures. Sensory processes, simple and direct associations, and the blundering advances of 'trial-and-error' were the popular terms in laboratory studies upon learning in man and in other animals. It was only a matter of time when the limits of these simpler categories of description and explanation should be discovered and when 'higher' processes and modes should be again invoked. Moreover, the recent occupation with the organism, in biology and psychology, as against the analytical inspection of parts, details, and elementary functions, has also contributed to this tendency in the study of learning. Organismal factors, *Gestalten*, the primacy of the general pattern, the influence upon development of the total-body, and the general directiveness of life, have all been facts of this same order tending away from the older and simpler mechanisms. It was natural that 'insight,' 'forecast,' 'purpose,' and even 'intuition' should have been rescued from the commoner and

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less precise employment of them as dictionary terms and turned to psychological account.

To psychologists familiar with current and recent discussions, it will be unnecessary to dwell upon the revival in periodical and textbook of the term 'insight' and to point out the variety of meaning and the resulting ambiguity in its use.[1] Some of these meanings are perceptive apprehension, acute observation, understanding, foresight and forethought, rapid learning, an intuitive flash, sudden grasp or illumination, intelligence, sophisticated skill, cognized relations, the felt basis of an attitude, experienced determination, a new perception of a goal, and a new configuration.

Now there are obvious difficulties in approaching by experiment a topic so uncertain and so variously shaded by theory and point of view. All we have tried to do in this study is to submit to experimental conditions various situations which have been commonly alleged to exhibit experiences and performances characterized by 'insight.' Here it has been our hope that the appearance of a common process, function, or accomplishment might help us to an understanding of this elusive characteristic. In anticipation of our results we may provisionally say that no such common and unique operation of the animal organism has emerged from our quest. Nevertheless, the experiments have thrown considerable light upon a class of performances just now under close scrutiny from many sides.

The Experiments

Most laboratory studies of 'insight' have been of the problem-solving and maze-running kinds and have made use of children, other primates, and white rats. In order to add the very important experimental control which is to be derived from the report of the observer himself, we have used human adults, both individuals trained in psychological reporting and individuals untrained. For both sorts we have added the device of technical *instruction* in order to obtain the actual conditions (commonly left—even in experimental settings—to inference or to speculation) under which the organism is thrown into functional commission. It would seem now to be of less importance to *infer* insight from behavior ('insight' taken, e.g., in one of the above senses) than *to obtain a descriptive account of the*

[1]Only here and there has a critical voice been raised. Examples are G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242-253, and S. C. Fisher, A critique of insight in Köhler's 'Gestalt Psychology,' this Journal, 43, 1931, 131-136.

actual modes of organic performance where insight has already been alleged.

We have taken individual observations and also, in the light of results thus derived, have secured observations from small assembled groups. We consider the problems in order, setting down the individual-results first, then the group-results.

Thirteen problems, presently to be given in a natural sequence, were placed before each of 7 *O*s. Five of these *O*s were trained in psychological observation. The other two were without technical training.[2] For the group-presentations, 110 students were used in congregates of 5-8 from an undergraduate course in general psychology.[3] In order to prevent troublesome influences from successive presentations, the following temporal order was adopted: Problems 1, 5, 6, 7, 3, 12, 2, 4, 10, 9, 8, 11, 13. The groups worked through only certain problems.

Procedure. For the individual *O*s, the formal instructions[4] were placed on a large blank sheet of paper which lay on a table immediately before the *O*. The table was in a corner with blank walls in front and to the right, while on *O*'s left sat the experimenter. Upon the table were usually scattered various objects, some having little or no relation to the problem, others supplying means for various solutions. The arrangement of these objects remained fixed throughout the experiment.

So far as possible, these arrangements were preserved in the group-presentation. Some changes were necessary. For the groups a large rectangular table was equipped with a single set of the same objects in a similar haphazard arrangement. No less than five and no more than eight *O*s at a time were seated on three sides, while the fourth was reserved for *E*. A pencil and mimeographed copies of certain instructions and all questions concerning procedure were

[2]*Observers.* The trained *O*s were Dr. S. Feldman (*Fe*), Dr. J. G. Jenkins (*Je*), R. B. MacLeod (*Mc*), members of the staff; E. H. Kemp (*Ke*), A. D. Glanville (*Gl*), graduate students in Psychology. The untrained *O*s were H. A. Myers (*My*), and K. M. Simpson (*Si*), graduate students in Philosophy and Physics respectively. C. M. Wiltse (*Wi*), and R. G. Fisher (*Fi*) were occasional observers.

[3]For the group-presentations, 110 observers from the undergraduate classes in Psychology volunteered. This number observed in all problems presented to the groups except problems 5, 6, and 10, which had 63, 50, and 64 observers respectively. Since the experimental period of each group was limited, it was found necessary to alternate problem 5 with problems 6 and 10.

[4]For the three main forms of *instruction* (formal, occasional and self) see O. F. Weber and M. Bentley, The relation of instruction to the psychosomatic functions, *Psychol. Monog.*, 35, 1926 (no. 163), 1-15; M. Bentley, *The Field of Psychology*, 1924, 389-396.

placed face downward before each *O*, to be turned over and read at a signal from *E*. Other instructions were orally given. All questions were to be answered 'yes' or 'no' except the last one of each set. Emphasis was placed on the fact that the experiment was not an intelligence test of any kind or a competitive trial of capacity. The grouped *O*s were directed not to handle the experimental objects.

At times during the experiment, however, it was found necessary to assure the grouped *O*s that a solution was possible, and this assurance sometimes evoked a self-instruction that 'I am being compared with others and must make a good showing.' These by-products of socialization—while they must never be assumed—must always be looked for in group-operations. They frequently determine output and function in the solo experiment as well. Only careful report will turn them up and estimate their influence. As antecedents and determinants of function they can never be behavioristically inferred from mere physical proximity and isolation.

Problem 1. This problem turned on the significant use of materials which might suggest some form of 'insight.' A string of beads was placed directly before *O*. It consisted of two small white 'pearl' beads alternating with one larger yellow glass bead, except for the middle portion of the string where five white beads separated the yellow. The bore of the white beads was too small to admit of stringing except by a single thread with a moistened and rolled end. The needles supplied were too large.

(*a*) *Individual Os.* Two sets of instructions were made, the second for presentation in case an *O* failed to obtain the solution under the first.

> *Instruction 1:* Make a single regularly repeated pattern without either unstringing or restringing the beads, and without knotting or breaking the thread. Proceed aloud.
>
> *Instruction 2:* Make a single regularly repeated pattern without either unstringing or restringing the beads, and without knotting or breaking the thread. Use any of the means supplied on the table. Proceed aloud.

Due to the emphasis of the first set of instructions, attention was primarily centered on the experimental object. Each *O* first sought to get the instruction clear and then carefully examine the beads in order to understand fully the conditions and limits of the situation. Twisting the threads together by some means or other and sliding the beads across was one of the most obvious solutions suggested. It never worked, of course; and the *O* soon realized that instructions stood against it. The superfluous beads could not be unstrung.

Four *Os* (*Fe, Si, My,* and *Gl*) went through a process of elimination to arrive at the solution. *Fe* solved by a consideration of various means of eliminating the irregularity, "I see nothing to do but twist the threads together. If they

would hold together, the beads could be slipped over." He picked up the beeswax. "It won't do." He picked up the glue. "No, no. That won't do. It is too sticky." He picked up the needle and sought to use it but found that the white beads would not go over it. From the first the various articles and tools available entered into the situation for this *O* as having possible significance. Now he picked up the saw from among the destructive instruments and laughed at using it. But after he had considered all the ways that the superfluous white beads could be eliminated, he suddenly asked, "Can you break the beads? That is the only way of getting them off that I can see," and he picked up the pliers and broke one of them. "Breaking was a last resort. I had tried everything else. I could think of nothing else until that occurred to me as possible."

For *My*, *Si*, and *Gl* the elimination took place without reference to the objects on the table but from the conditions imposed by the instructions and limitations presented by the object itself. "The beads had to be removed. Since all the natural ways were prohibited, that was the only one left. I tried first to crush them with the fingers but they were too hard. I thought of biting them but that was bad. I looked around and saw the tools and I took the pliers and broke them" (*Gl*). It is obvious here that the instruments played a minor part in the solution. They merely furnished excellent, because stronger, substitutes for hands and teeth. The *O* was set for an instrument to break or crush when he looked at the various materials on the table. "If you cannot move any bead over another bead, you cannot possibly change their relative positions on the string, and they are an uneven number, so they cannot be distributed so as to make a regular pattern. Can the thread be passed through any bead twice? But that would be hard to do. Are you allowed to break the beads?" (*Si*). He then counted the beads and said, "That would do. Why I thought of breaking the beads, I do not know. I just did. It was the only way of doing it." Here again it was only after the correct method of solution was obtained that the instrument for its accomplishment was sought.

My, proceeding step by step, commented clearly and logically, "Two-one, two-one is the pattern. There are five extra white beads. If those five were out, it would be a regular pattern. One might make one by distributing the white beads. How can they be distributed? There are five white beads and twenty-one large ones, therefore they cannot be evenly distributed. The directions say not to unstring or restring, etc.; but those beads cannot be distributed without breaking or knotting the thread. I can try arranging the beads on the paper. Thus far I have been thinking of the pattern on a straight line. I can make a circle or a figure like this, but that won't succeed. Is it permissible to break the beads? The instruction does not say anything about not breaking the beads. I can do that. . . . That was the only possible solution because, even if you could break the thread or did break the string, you could not make a regular pattern utilizing the beads."

No reference in these two cases was made to the instruments lying on the table. They were as completely extraneous to the solution of the problem as if they were not there. But in the actual breaking the *O* invariably surveyed the outlay and seized the pliers for the purpose. That this instrument easily became a destructive crusher in this situation is not surprising, when one considers its manifold uses in constructive building where it is often used to break or bend.

Three *Os* (*Wi*, *Je*, and *Mc*) failed to reach the solution. It never occurred to *Je* and *Wi* that the integrity of the beads could be destroyed in any way. The beads were to be removed or redistributed as beads. They never got beyond this assumption, and this assumption was never formally recognized. That all *Os* were inhibited in some measure—although not so completely as with *Je* and *Wi*—against destruction as a means of solution is obvious from the fact that after a consideration of many possible methods of removing or redistributing the intact beads, the *O* either declared it to be impossible or asked if it was not so. At this point he was invariably assured either that "it was not impossible" or that "the irregularity could be eliminated." This is a clear instance where *the occasion* (occasional instruction) stands directly against the carrying out of a formal instruction.

The most interesting case of inhibited action was that of *Mc* who failed to reach a solution under either instruction. Under the first instruction attention was focussed, as in other cases mentioned, on the beads and the limitations imposed by the conditions of the instruction. After various suggestions in attempting to make a uniform pattern, and after having been told that such a thing was possible, he "does not see how it can be done." Under the second instruction, he picked up the saw, "May I use this?" Then he picked up the thread, "This is the only thing that might do." He took up the objects one by one, remarking each time "That won't do" or "This does not seem to be applicable"; but, except for the saw, he never considered the destructive instruments at all. "I'm still utterly baffled by the situation. I see only one way of making it a pattern and that is barred by the instructions."

That *Mc* noticed the instruments was evidenced by his handling the saw; but that they were recognized only in their usual contexts and not in any unusual relation to a delicate string of beads was apparent from his behavior as well as from the above account. This is the opposite of what occurred in the case of *Ke*. The mere presence of the instrument within visual range caused him, intent on some means of removing the superfluous beads, to give consideration to any possible relation it might have to the problem in hand. After examining the string and trying to run another string through the beads with the needle and thread on the table, and after the assurance that the solution was possible, he leaned back, looked at the table, and said, "I might break the beads and take them out. . . . The beads could not be moved according to the instructions. The large ones could not be slipped over the small ones. I saw a pair of pliers over here and saw the string of beads over here that could not be moved off. I just re-read the instructions and broke them." The instrument for *Ke*, as for *Fe*, entered into the situation from the first; but for *Fe* 'breaking' suggested the instrument while for *Ke* the instrument suggested 'breaking'.

Results. Acting under occasional and self-instructions and anticipatively apprehending the results *O* eliminated one plan after another until the solution either was reached or was blocked by an interfering occasional instruction or by the failure of comment and imaginative apprehension. The elimination of possibilities may take place either with respect to suitability of means or instruments or

with respect to procedure permitted within the limitations imposed by experimental object and formal instruction.

For the *O* there is always a plan or method of approach. When his approaches all fail him, he ceases to try and desires to terminate the experimental period. He never continues, trying at random, in expectation that the solution will simply occur. He always applies a definite method and this method is a contribution of his own under self-instruction. He does not wait for sudden appearance of the solution. He does something that definitely brings about, or causes, or creates that solution. There is always an accompaniment of pleased pride in, and approval of, himself with the accomplishment of the solution. In failure there is frustration combined with impatience and disappointment with himself.

Inability to see an ordinary object in an extraordinary context and a predisposition to respect the integrity of property constituted frustrating moments.

(*b*) *Group Os.* The instruction was the same as Instruction 1 above, except for the direction "Proceed aloud" which was omitted. The time allotted was 10 minutes. Then the following questions were answered. (The replies are given in percentages.)

		Yes	*No*
1.	Did you look over the table to see what you could use?	14	86
2.	Did you consider the string of beads only and what the instructions did and did not allow you to do to it?	93	7
3.	Did you solve it?	25	75
4.	Did you rearrange the beads to form a different kind of pattern?	42	58
5.	Did you break the beads (imaginatively)?	25	75
6.	Did you think of breaking the beads first and then look for the proper instrument?	12	88
7.	Did the instrument make you think of breaking the beads?	3	97
8.	Comment further upon your procedure, telling any way you went about eliminating the extra beads that is left out in the questions.		

Since a quarter of the group (Question 5) reached the 'breaking' solution and only 15% of them (Q. 6 and 7) considered the instruments, it is obvious that a tenth of them who thus solved did not regard the instrumental means. But then, they were not actually called to smash as the individual *O*s were. This is an interesting difference in solution, which is also supported by Questions 1 and 2 (concentration on the beads). Answers to the last question indicate that practically all the methods employed toward solving by this large number were also employed by the smaller number of individual

Os. Pattern arrangement, bead counting, and the consequent deduction that redistribution was impossible were means suggested.

In a few cases the solution came immediately. Those who suggested breaking the beads report substantially as follows: "The instruments had nothing to do with it either before or after getting the solution. I took it for granted that the tools on the table were for another experiment." "I thought of breaking them with my teeth or the eversharp in my pocket." "I never thought of the instruments on the table. When I saw the table, it seemed more like a class in vegetable gardening than an experiment"—another good example of apprehension of an object only in its usual context.

Some, after making several tentative and ineffective suggestions, concluded the solution impossible. Most of them, however, failed to get beyond a stage of search or of comment-and-inspection in the time allotted.

These results substantiate those from the individual *Os*. Inhibited solutions are more plentiful, due, perhaps, to the small amount of time; but the initial steps of the solving process—apprehension, inspection-and-comment, search for method, and procedure under self-instruction until that instruction is perceived as inapplicable, re-apprehension and search for another method—are typical here also.

Problem 2. This problem was intended as a parallel to the foregoing. Small white wooden beads were obtained, and some of them were stained black with India ink. They were then strung with the white, the pattern alternating one white with one black, one white with one black, except toward the center where eight whites separated blacks. The solution lay in converting the ink in the bottle on the table to the uses of a dye. No *O* questioned how the black beads were colored but accepted them as given.

Instruction 1. Make a single regularly repeated pattern without either unstringing or restringing the beads, without knotting or breaking the thread, and without destroying the beads. Proceed aloud.

Instruction 2. Make a single regularly repeated pattern without either unstringing or restringing the beads, without knotting or breaking the thread, and without destroying the beads. Use any of the means supplied on the table. Proceed aloud.

If the *O* failed to solve under the first set of instructions, the second was presented.

The solution involved the use of a medium in unusual contextual relations. In some cases it was obtained more easily than in the preceding bead-experiment. The *O* was memorially instructed from cues gained there. "I got the solution of the present string from a cue from the other beads. As soon as I got away from the rearrangement of the beads on the thread, it was easy to think of doing other things" (*Mc*). "I am looking for possible interpretations of your instructions" (*My*).

Je, *My*, *Gl*, and *Mc*, as in the preceding experiment, set about seeking the solution, not from a consideration of the whole experimental situation, but from

the conditions imposed by the instruction and the experimental object itself. Search for a way of attack, accompanied by a careful inspection and comment upon the situation, was characteristic of the procedure. *Si*, for example, asked about the instructions to make sure he had them accurately and picked up the string and examined it. "There is no catch on the string." He examined the string and beads again carefully. "One thing is obvious; if there are more white than black beads, you must either get rid of some white ones or else get some black ones. There are no black ones. Is this string symmetrically arranged with regard to the center ones? No, it isn't." He read over the instructions again. "For all purposes this is a closed circle and no bead can be moved over another nor can they be broken. Is it unsolvable?" When answered negatively he said: "Well, something must be done to the white beads."

Where the solution was reached, the *O* invariably arrived at it through a consideration of painting the beads. "I did not notice the ink on the table, but I named ink instead of dye or paint because I had had more experience with ink than with either of the other two" (*Mc*). "How about some paint or something? Some black ink if there is any available," he picked up the ink bottle, "Or, better still, the black ink from my fountain pen" (*Si*). "I thought of paint first but then I thought of ink because you would be more liable to have it around" (*My*). Here again the method of solution was obtained before the exact medium for its accomplishment. Since the *O* searched for something he could use in following out his solution, ink easily became, under these circumstances, a dye in spite of the penstaff lying alongside the bottle.

As before, *Fe* and *Ke* derived the solution from the whole experimental situation, taking into account every article on the table and its possible relation to the problem in hand. *Ke* ran his eyes rapidly around the table a few times inspecting and commenting upon all the objects on it. "Suppose I run another string through and pull this one out. . . . But I guess that is unstringing and restringing. Oh well, I can paint them black," and he seized the ink bottle to do so. *Fe* followed much the same procedure, glancing all around the table and then reaching for the ink bottle. Here there was a very definite instruction resulting from the solution in the former experiment.

Gl and *Je* failed to solve the problem, the case of *Je* exactly paralleling that of *Mc* in the former experiment under both instructions. *Gl*, however, solved it under the second instruction, "It said, 'use any of the means on the table.' I had tried all the arrangements of the beads before and they would not work. I thought there might be some chemical here that you could put on them to change the color. I looked around and saw the ink bottle and thought, 'Aha, that will do it.' "

Results. Here each *O* followed the same general procedure as in the preceding experiment. If he had limited himself before to a consideration of the experimental object and the conditions of the instructions, he did so in this case; but if he had taken the instrumental approach in the first, he followed it now. Only one exception occurred. The change from the first instruction to the second in the case of *Gl* induced a change in the set toward the problem.

In some cases *Os* took cues from the former problem. Practically all of the inspection of the experimental object and weighing of the instructions were absent here from those who approached the solution by an instrumental elimination. Self-instructed, the *O* looked around the table, immediately questioning the suitability of the various objects. Cues were of aid, too, in the other approach, where the *O* paid more careful attention to possible implications in the instructions, and where he took account of and guarded against any inhibiting moments similar to those in the former problem.

But in other cases helpful cues were not carried over. The *O* was as strongly inhibited here as before by his custom of seeing a thing in a useful context from apprehending it in another and a newer one. In one case (*Je*) the cue from the previous problem gave him a set toward destruction as the means of obtaining a single uniform pattern, a set that was useless in the solution. Even under the new instructions, this set was carried over.

The same elimination of one self-instructed plan after another, the same methodical procedure of search accompanied by inspection-and-comment to the formulation of the self-instruction, and, when no other plan could be made, the refusal to continue planless characterized this problem as definitely as the first.

Problem 3. A cork in a large bottle with a very narrow neck was presented to *O*.

> *Instruction.* Remove the cork from the bottle without destroying either the cork or the bottle.

The situation here differed from the first two in having the solution by instrumental means suggested. A large corkscrew, the usual means of removing a cork from a bottle, lay among the tools, which also included a small flexible wire. While this suggested the use of an instrument, however, it was always that particular instrument. Only one *O* analyzed the situation to the extent of not applying the corkscrew first. *My* sought to gain a thorough comprehension of all the conditions inherent in the experimental object. Inspecting and commenting minutely upon the cork and bottle, suggesting one means after another without reference to the tools, he finally remarked with characteristic acumen, "Judging by the appearance of the cork, I believe mechanical means have been used on this before," and seized the wire, knotted an end which was too small, made a larger knot, caught the cork on it, and pulled it out. *Ke* picked up the wire first but threw it down without trying it, "I must try the corkscrew."

All the other *Os*, after trying the corkscrew, made a loop of either the wire or the thread, caught the cork at the right angle, and with and without the aid of the corkscrew, pulled the cork out. Here as before a means is employed effectively out of its usual context. No *O* failed in this solution for various reasons; first, because corks inside bottles are a common occurrence, and, secondly, the experi-

mental set-up favored the correct solution. When sharp point and corkscrew were not successful, the *O* invariably seized upon the wire.

Results. When the occasion suggested the correct direction in which solution was brought about, there was a tendency on the part of the *O*s to go in that direction. In most cases, however, the more obvious suggestive factor, the corkscrew, inhibited a consideration of less obvious but equally suggestive factors (*e.g.*, those considered by *My*) until failure brought about a further investigation of the experimental situation with another consequent plan of procedure.

The same inspection-and-comment, the same search with an eye to possible ways of procedure, the same methodical self-instructed plan of procedure and elimination through failure or inapplicability characterized this problem as the others.

Problem 4. This problem consisted of a series of seven cards presented in order bearing seven different arrangements of the line-

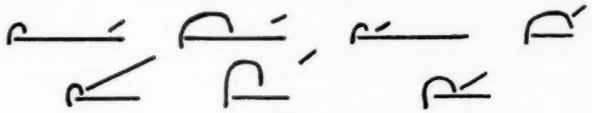

FIG. 1

elements of the letter R lying on its side, but the last arrangement comprising the R as it is ordinarily presented except for its sidewise position (Fig. 1). The principle of this series was suggested by problems in the classification of a new species or variety in taxonomic zoology or botany. The aim was to lead the *O* through a number of steps containing very familiar elements in more or less strange relations to each other to these same elements in their familiar relations. Experimental interest was centered on how soon the *O* saw the familiar relation, and, after seeing it, commented upon the whole series.

Instruction. Identify each member of the series presented to you.

The element of search was quite apparent in most of these observations. "I keep wondering, 'What do you want?' " (*Ke*). " I can name the lines. It seems as if it ought to represent something more definite than that" (*Gl*). "Nothing like sea-lions, is it? Is it a joke or anything of that nature? I try to see something there, but I am not sure of it. It is not supposed to be an ideograph, is it? Is it supposed to be something you can see?" (*Si*).

One thing after another was named by the *O*s as a possible identification; although in practically all of their observations there was an element of uncertainty concerning the object with which it was identified. "It looks like the other figures. It might be two or three objects like tombstones, etc., except for faults"

(*Ke*). "It looks like a P on the side with a long vertical part. What the little oblique line is bothers me. It is also a stream of water in a tile" (*Gl*). "It is a railroad track going into a tunnel. I cannot make out the slanting line" (*My*). "It is a mouse that is only partly there. The tail and head are missing" (*Si*).

In every case but one the *O* sought to give a meaning, though often very unsatisfactory, to the figure—to make it a symbol fitting in with known and recognizable phenomena. Only in the excepted case (*Mc*) very little effort was made to name the figure as some familiar object. In this case the object was too uncertain, too unsatisfactory. He comments: "It does not look like anything to me. It looks like a tunnel, a road here, and a soldier." "That is nothing at all." "Nor that. It requires an awful strain to get the soldier and tunnel."

There was a strong tendency on the part of many of the *Os* to analyze the lines before naming the object with which they might be identified. "It is just a curved line, and a straight one, and a short one" (*Ke*). "One is a straight horizontal line, the other a curved one, and the other oblique. . . . It is again like the other one but this time there is a short horizontal line, with the curved and oblique lines above it" (*Gl*). "I cannot make out the slanting line. The horizontal line is longer" (*My*). "It is a poorly drawn semicircle. It is like the first thing except the curve is longer and the little oblique line higher" (*Fe*). In general, these *Os*, who saw the figure as a group of lines first, tended to make the letter identification long before presentation of the last card; although this identification was, for the most part, as uncertain as the object identifications.

Fe, *Gl*, *Si*, and *My* suggested a letter early in the presentation. The first figure for *Fe* is "a sort of P lying on its side with a long stroke," and the second is "like the first except the curve is larger and the little oblique line higher. It suggests an incomplete R." For this second one also *Gl* commented "If I look at it sidewise, it suggests an R with part of it missing." For the fourth, "The curve and the horizontal line suggests an incomplete D," but for the sixth he said, "It looks like an uncompleted R. When I take them (the lines) as water in a tile, the oblique line does not fit. When an R, it does fit." *Si*, in the midst of positing several objects which the lines might represent, said, concerning the third figure, "If I turn it around, I can make an R out of it but I guess I'm supposed to keep it upright." He was self-instructed here to avoid the unusual in position. On the fifth figure *My* commented, "The line is longer, the tunnel is smaller. It looks like the letter R. I turned my head sidewise and looked at it like that. Once before it looked like the letter R rather remotely," and, on the sixth, "It still looks like the letter R."

For *Ke*, *Mc*, and *Je* the letter appeared only on the presentation of the last figure. Failure to suggest an R earlier was ascribed primarily to the set. "It is an R—a letter. The elements were so scattered in the others that that must be why I never got it before. I never thought of a letter" (*Ke*). He picked up the cards and went through them: "It is easy to see them all as Rs now." "The whole series is composed of various distortions of the R turned sidewise. I was set for a picture. I did not expect anything tipped over" (*Mc*), and for *Je* the set was strong enough to cause him to continue the pictorial identification to the last figure, "The first flash is a rabbit, the semicircular thing representing an ear. I just decided this when I got a very brilliant flash with an incipient shrug of the shoulders, 'Why, of course, that is the letter R.' There never was in any of the

earlier figures any suggestion of a letter. I have some suspicion that either the card tipped as you put it down, or my head did. You can see that the same motif was carried out throughout" (*Je*).

Care was taken on the part of *E* to prevent tipping the card. This "kinaesthetic pull toward the left," "tipping or turning the head sidewise" was mentioned frequently in the commentary, not only for a letter, but for objects in motion. "There is a kinaesthetic pull in the shoulders toward the left. I have a visual apprehension of the long angular line travelling toward the left" (*Je*).

From the final reports of the various *O*s it was obvious that every member of the series had taken on the characteristic significance of the R symbol. The purpose of every member now was to represent that letter. "We have our R this time. It is a tied image R, too. The R stands out immediately. There was no regular development in the R, but I can see now that that was what it was all along" (*Gl*). "There is the same general character to the whole series. All of them represent an R quite well, although each of them taken individually could have represented any of the other things equally well. But you can see now that it could not have been anything but an R" (*Si*). *My*, who was sure of the letter on the fifth figure, reported, "I think I noticed the R on the third one. There was not a close enough resemblance. I thought I would see a few more before I committed myself."

For the most part, the same general elements were noted from presentation to presentation, but their direction was undetermined. That "one figure is like the preceding one" or that "there is a certain similarity among them all" was reported time and again; but the significance was never fully determined, except in the case of *My*, until the last and most familiar relationship was presented.

Results. The reports from this problem display every stage of advance toward identification from an almost pure perceptive level, accompanied by the vaguest and most indefinite kind of puzzled search, in some cases, to a more concrete, though tentative, symbolization for some familiarly recognized object or objects, in others.

A uniformity among differences was one of the first general conditions noted, but an uncertainty of its significance accompanied the comment of every observer but one until the last member in the series was presented. Then the significance became certain either immediately or secondarily, depending upon the set of the *O*. After once knowing the correct symbol, a recognition of its appropriateness in comparison with any other identification occurred. This revaluation of the whole series in the light of the last member was characteristic of every *O*.

By a process of finding the most appropriate identification-object, an object which fitted in best with the recognized uniformity, two *O*s named the correct letter midway in the series, the later presentations merely serving to confirm the conjecture.

The disposition on the part of the *O*s to verticality, to apprehending objects in their customary positions, was an inhibiting moment in the identification of the lines as a letter. So also was the predisposition to regard the lines as a picture of something.

Those *O*s who inspected the members as lines first tended to name letters as well as objects for identification early in the presentation. *Ke* was the only exception to this.

Problem 5. This problem was intended to demonstrate a solving procedure combining 'trial and error' with 'insight' (= 'foresight and forethought' or 'new configuration'?). A set of papers, each

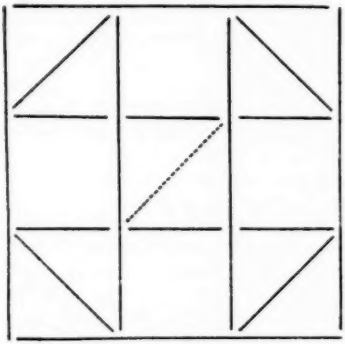

FIG. 2

sheet bearing Fig. 2 (but wanting the dotted diagonal at the centre) was presented to *O*, who was supplied with a red pencil for clear retracing of the lines. Gaps at certain junctions were left to discourage retracing. A clear disjunction of the inscribed figure from the surrounding rectangle was hinted at in the drawing.

Instruction. Here are two figures. Trace continuously (without retracing) every line of both. Use just as small a number of copies as you can. You will be penalized for every wrongly traced set. Comment aloud.

(*a*) *Individual Os.* Here as elsewhere conditions were considered and no random stumbling trials were made.

Search for the best way of attacking the figure, and inspection of and comment upon its salient characteristics with reference to the most advisable way of beginning marked the procedure. The *O*s had concerted plans before placing pencil on the figure. "I looked to see how the two could be traced independently. I saw that the square could be gone around simply enough. So could the other figure" (*Ke*). "I did it mostly by means of eye-movements. The solution came practically immediately" (*Mc*).

Some *Os* traced the figure first by moving the eyes over it. Others traced it in the air with the pencil held above the figure. Most of them began the actual tracing on the inside figure; but *Gl* and *Wi* began at points on the outside square. The beginning points were based on occasional instruction *plus* inspection-and-comment. "I saw that I must begin at a point here and I saw that, since the figure is symmetrical, it made no particular difference which of the like points I did begin on" (*Si*). "The outside figure must be begun at a point on the inside one" (*Gl*). "The first glance revealed that a gap would have to be left in the corner of the square which would have to be done at the end. Then the inside figure could be drawn immediately" (*Wi*).

After reading the instructions, one *O* had difficulty in seeing the two figures. "At first the gaps and the two-figure idea bothered me. I saw it first as a square and an octagon with other line-figures" (*Mc*). "I am looking to find the two figures" (*Si*). In general the *O* began by seeking to understand the instructions fully in order not to miss any condition which might be of significance in the given problem.

Results. After a mastery of the instructions and after inspecting and commenting upon the experimental object, the *O* approached the solution by a method of action under self-instruction and perception of its results, that is, of starting from some plausible point and proceeding until balked by an unforeseen condition. Thereupon another point was chosen and the same procedure continued, always, however, with this condition taken into account. This method was followed until the solution was reached.

Owing to the threat of penalization, the figure was concretely traced only after the solution was obtained.

(*b*) *Group Os.* The same figure under the same instruction, but without the direction "Comment aloud," was presented to the group. Five minutes was allotted for actual tracing.

Questions	*Yes*	*No*
1. Did you see the two figures as a square and the figure contained in the square?	92	8
2. Did you begin to trace the figures immediately?	14	86
3. Did the threat of penalization in the instructions keep you from immediately tracing the figure?	44	56
4. Did you look it over before you began to trace it to see first how it should be done?	89	11
5. Did you hold your pencil in the air while you went over it?	56	44
6. Did you begin on a corner of the inside figure?	52	48
7. Did you begin on the outer corner of the inside figure in order to get from it to the square?	49	51
8. Did you begin on the square?	52	48
9. Did you begin on the square at a point where the inside figure touched it so that when you had finished the square, you could get inside?	41	59
10. If you used more than one copy, did you decide to quit this first one because you could get no further and would have to start over again in a different place?	2	
11. When you began to trace, did you do so deliberately?	87	13

12.	Were you looking ahead to see where your movements were leading you?	83	17
13.	Did you trace it swiftly?	21	79
14.	Had you already determined the correct procedure so that you could trace it swiftly?	17	83
15.	Comment further upon the whole of your procedure, filling in details omitted by the questions.		

Eighty-two percent succeeded in tracing the figure correctly. Except for the last four questions, where some confusion seems to exist with regard to the method actually pursued, the procedure parallels to a large extent that of the individual *Os*. Many *Os* report in answer to the last request that they were "unable to finish the figure because of the time" or else that they "had not time to consider thoroughly, in order to do it correctly." That most of the tracing (trying out some self-instruction) was done, not on the paper, but above it, is obvious from the answers and from such reports as the following. "I mentally traced it." "After looking it over, its simplicity was evident." "The method of determining the procedure was done in my imagination rather than attempting it on copies."

Action, not a blind headlong sort of action, but from a self-instructed initiation, is characteristic of the procedure. A reason of some kind is reported for every attack. "The square could be traced starting from any point." "I first looked over the whole figure and decided that I should start with the inner figure and then trace the square, which is one continuous line no matter where you start from. I planned how to trace the inside figure and to get from it to the square without retracing. Then I traced it."

The general procedure with individuals and groups alike may be said to have been characterized by a period of search and of inspection-and-comment leading to a self-instruction and action under this instruction, followed by perception of the results.

The figure was repeated on fresh sheets with the dotted diagonal of Fig. 2 now added as an unbroken line. The task was therefore considerably changed.

Instruction. A line has been added. Trace all lines again under the same conditions.

(*a*) *Individual Os.* That the added diagonal was a further complication involving more effort for the solution was immediately recognized by most *Os*.

"This complicates it" (*Si*). "This will take longer" (*My*). "This complicates matters a bit" (*Wi*).

Gl, *My*, and *Si* traced again with the pencil slightly elevated from the sheet, while *Wi* "went over it mentally twice and then drew it."

The reports of no two *O*s were alike and yet a certain uniformity was evident in the procedure leading to the solution. In all cases but one some reason was given for the beginning point and the point of departure from one figure to the other. This one exception reports, "I have decided that I cannot begin where I did before but I will have to begin at the center. The solution seemed to come about naturally" (*Mc*). Most *O*s traced their figures, reporting their procedure afterwards; but one *O* commented while tracing it in air; "If I start on the diagonal, I must take care of the other two lines by crossing. I have to end on one point of the diagonal if I start on the other" (*Si*). Another *O*, who never traced the figure except with his pencil on the sheet, commented: "This has the same figure as the other practically," and, "When I started, I could not swear that the beginning on the middle diagonal would be successful. I knew the square could be traced continuously and that the figure in the previous case could be traced the same way. When I first put the pencil down, I knew that I could get practically all around, but I was doubtful about the corners. When I got to the last corner, I paused and saw that I must do the square and come back to the last line of the inner figure" (*Ke*). "I knew I had to begin on the middle diagonal because I could not reach it without retracing the lines if I began on the outside of the figure. That I would have to leave the inside figure and do the outer one before finishing the inner came because I knew the middle diagonal was substituted for the one I began on in the first sheet" (*My*). "I began at the diagonal because I had just been over the same figure (except for the diagonal), and when the new line was added, the easiest way of getting it in was to begin on it. It was quite obvious that you had to do the square before you finished the middle figure. Otherwise you would never get to the square. If I began on the inside of the middle figure, I would have to end there and there was no way of getting from there to the square" (*Wi*).

The middle diagonal, the new element, commanded primary attention and drew interest away from the outer figure to the inner. Not one *O* attempted to trace the square first, and only one attempted to begin on the inner figure at any point apart from the diagonal. *Gl* held his pencil suspended from the paper, attempting to trace it, "Go from here to here to here, etc.—That won't do.—The square in the middle puzzles me.—I have decided to start at the same point I did on the former sheet." He was supplied with another sheet which he never traced until he had first gone over it in air, "trying to see the whole figure traced before I start. I got stuck because I kept thinking that I ought to arrive at the starting point. The idea of penalization holds one back." Self-instruction is thus an inhibiting factor in the solution as well as an aid.

Results. Apprehension of the object, search, and inspection-and-comment, leading to action under one self-instruction after another were the essential characteristics of the procedure. The instruction of penalization for every wrongly traced sheet caused most of the trials to be carried out either by eye-movement or by tracing in air. In some cases where the solution was not quickly forthcoming, the figures were so traced again and again. As a result, a sureness and

swiftness marked the tracing of those who had solved it to their satisfaction beforehand. Their reasons for this or that movement in order to avoid error were already determined.

On the other hand, those who started to trace before having well in hand the proper way of doing it moved the pencil slowly, glancing ahead to study the whole figure before crossing or turning at some critical point. This looking ahead for possible errors and correct ways of procedure simultaneously with tracing is obvious in the report of *Ke* whose tracing time was 30 seconds,—the longest time except for *Gl*, who, though he traced the figure again and again in air, was still uncertain when he began to trace the actual figure.

(*b*) *Groups Os.* The same figure was presented to the groups under the same instructions. The time allotted was 7 minutes.

Questions	*Yes*	*No*
1. Did you consider this task more difficult and complicated than the preceding?	93	7
2. Did you begin to trace this figure immediately?	14	86
3. Did you run your eyes over these figures before tracing to see how they could best be traced?	93	7
4. Did you go over the sheet holding your pencil suspended until you got the successful way of doing it?	72	28
5. Did you begin on the inside figure?	78	22
6. Did you begin on the middle diagonal of the inside figure?	53	47
7. Did you begin on one of the outside points of the inside figure?	19	81
8. Did you begin on the square?	26	74
9. Did you begin on the diagonal in order to get it in?	41	59
10. Did you leave the inner figure before finishing it in order to do the square and then come back to the inner figure?	55	45
11. Supply any information that is not covered by the above questions.		

Fifty-seven percent succeeded in tracing this second pattern. Of the others, only two *Os* traced more than one copy. The others either had not time to do so, or else the end of the period found a few of them still searching for a successful way to proceed on the copy first supplied.

The reports in answer to the last request were scanty in this portion of the problem. They were, for the most part, explanations of why the figure was not correctly traced. Too little time given and too much time spent on trying to find the most advantageous starting point are examples. Comments paralleling those in the preceding figure were given. "I did one hurriedly to see how it would work out and I did it wrong. I saw my error and drew the second one correctly by using the lesson I had learned in my first one." "I slowly traced the figure, not having any predetermined course, and was careful not to pass over any intersections that I could not come back to. When about half completed, I saw the way to finish the problem." "I tried it once and got everything but the middle diagonal. It was merely a repetition of the other figure. So I started with the diagonal and got it out of the way."

The results in the main confirm those preceding. That the majority of the *Os* followed the same procedure as the individuals is evident from the comments and from percentages. Many *Os* never got beyond the earlier stages of apprehension, search for a procedure, and inspection-and-comment upon the object. Others, going further, acted under one self-instruction after another until the desired result was obtained.

Problem 6. This problem, based on the method of multiple choice, was designed to show 'insight' (= comprehension, sudden grasp, cognized relations?). Fourteen rectangular pill-boxes were glued side by side to a long narrow board. In one box a bit of cotton was hidden. The board was removed from *O*'s sight each time the location of the cotton was changed.

(*a*) *Individual Os.*

Instruction. There is a bit of cotton in one of the boxes. Find it with the quickest possible dispatch, giving all your procedure aloud.

This problem afforded a very striking example of one self-instruction replacing another after having been upset by the experimental conditions. At no time, not even to begin with, did the *O* open the boxes with no plan. "The best thing is to open the boxes one by one from left to right" (*Mc*). "There is no way but to go through the thing methodically" (*Fe*). Even in the cases of the unsophisticated *Os*, where the formal instruction was obeyed quite literally at first, a definite system of procedure was followed. One *O* (1) opened all the closed boxes beginning at the left; (2) opened with both hands but began at the opposite end. "If this is going to run on for some time, I am going to begin to look for a system. Now that was in the third from the left. There must be some trick about it, so that if I get the right idea, I can go directly to the correct one" (*Si*). This system, self-instructed, varied from time to time as the occasion favored first one and then another. This is better illustrated in a later stage of the comment. (11th trial) "Last time it was third from that end. This time it is third from this one. That doesn't work every time. Maybe there is no regulation." (12) Opened one but it was not the correct one. Opened another and found it. "I am formulating a theory." (13) "It ought to be in this one by the theory, but it is not, and I hereby discard it." This *O* never worked at random although he recognized the uncertainty of his plan. "I may be on a totally wrong track trying to figure these out from the arrangements but I think it is a sensible way of doing it" (*Si*).

The pattern arrangement was the outstanding method of approach to most *Os*. "It occurs to me that I am getting some kind of pattern-test (like the animals') which I am to learn in the course of the presentation" (*Mc*). "I will begin at the left, examining each box as I go. It is in the third I opened next to three empty boxes" (*Ke*).

Any number of cues were tried—the pattern, marks on the boxes, the total number of boxes from one end, either end, and both ends, the number of closed next to empty boxes from either end, the number of boxes moved farther from either end at successive times according to a scheme, the same box every time,

etc. The experimental period was marked with pauses while the *O* sat looking at the boxes, noting their position and arrangement, and striving to memorize them.

One *O* remarked that he "must look out for the danger of making it more complicated than it was." Nevertheless he reported at the end, "I made it complicated at first. I thought of all sorts of possible methods" (*Gl*). "I looked for something exceedingly difficult" (*Mc*). Self-instruction toward the difficult was not apparent in any of the other *Os*. *O* merely took advantage of what the occasion had to offer whether difficult or simple. *My*, who was in general the quickest and keenest to take note of all possible relevant conditions, commented, "It must be some simple system because it does not take you long to fix it" (*My*).

The process of elimination of one self-instructed method after another as the conditions changed was most obvious and most logically organized in the case of this same *O*.

1. "There it is." (After opening the boxes systematically from left to right.)
2. "I do not see any sense to it at all. I cannot find any order to it. The first time it was near the three thumb tacks, next it was third from the end. The more boxes I open, the less credit I get, I suppose."
3. "This time there are no threes to go by at all unless I go by the number of closed boxes." (Opened the third closed box from the left and found it.)
4. (Opened them by hunting for the pattern of three. Opened nearly all of them.) "I know that it has some connection with three. It was in the center of the only three closed boxes together."
5. (Opened the one near the group of three open boxes. Opened another and found it.) "I have the wrong pattern." (Examined the thing carefully and made a mark on one end of the board.)
6. "I marked it because I thought it might be in the same box every time, but it was not." (He counted three from the other end and got the cotton.)
7. (Opened from a cue from last time.) "Why do you have more lids on the boxes?" (Opened again and again and found it.)
8. (Opened a wrong box. Opened the right one. Counted the number of boxes.) "I wondered if you had moved it over one, but you had not. This time it was in the center of the pattern as it was two times ago, but that does not hold for all times."
9. (Looked where he did last time.) "Did you move it over three? Would it be a certain number of covered lids from either end? But figures do not prove it." (Then he found it.) "It is in the third from the end."
10. (Looked at the boxes.) "You do not move it over a certain number of times nor does the number three have anything to do with it, nor is it the center of a pattern, nor is it in the same one every time." (Tried the same box, but from the other end this time. It was there.)
11. (Opened the correct box.) "It is the third closed box over."
12. "I thought that when it was five from one end one time, it would be at the other end next time in the fifth box. I have never found it in an end one. I am trying to make sense of the arrangement. This time it is the sixth closed box from one end, and the third closed from the other."
13. "It must be some simple system because it does not take you long to fix it."
14. "Why is it never in the end ones?" (Counted the boxes.) "Next time I shall count the number of open ones."
15. "It certainly is no pattern."
16. "And it is not the same design this time. Apparently you do not have any system, but it has been in this one at other times." (He had opened the third closed one from the end and got it.)
17. "It is the third closed box from the end."
18. (Opened the third closed one from the right.)
19. (Opened the third closed box from the left.) "They alternate." (*My*).

This protocol is a fine example of the typical procedure in this particular problem. It is rich in illustrations of apprehension of arrangement and place, of search for some plausible ground on which to base a choice, of inspection-and-comment upon the various experimental conditions, of the formulation of the self-instruction dependent upon the occasion (arranged boxes) and action under it, of re-apprehension followed by another self-instruction and action under that, until, all irrelevant plans eliminated, the solving generalization is reached.

Results. The procedure in this problem, as in the others, was marked by a method, a systematic series of interacting self and occasional instructions. A solution was gained through, first, inspection-and-comment upon all conditions possibly relevant to the end sought, and, next, formulation of one self-instruction after another dependent on some occasional instruction until the generalization was reached. Here, again, there was no evidence of any uninstructed and planless activity. Action was always directed, predetermined, relevant to some intention on the part of the *O*.

(*b*) *Group Os.* The scheme for group-presentation consisted in placing the cotton always in the second closed box from the right end. It was necessary to make the scheme simpler than the *alternating plan* used above since the number of *O*s was greater and the presentation-time 10 minutes less.

Instruction. I am going to present to you this series of boxes a number of times in succession. In one of them there is a bit of cotton. On the first presentation, I shall open the box in which the cotton lies. After that you are to write on the paper before you the number of the box in which you think it lies, counting from the right. After you have written down the choice each time, I shall open the correct box.

Questions	*Yes*	*No*
1. When the boxes were first presented to you, did you consider that the best way of finding the cotton was to open all the boxes to see where it was?	52	48
2. Did you try to remember which box it had been in from time to time?	74	26
3. Did you try counting the number of boxes from an end?	80	20
4. Did you try to remember the pattern arrangement from time to time?	60	40
5. Did you try counting the number of closed boxes from either end?	42	58
6. In naming the box in which you thought it might lie, was your reason for the choice based on some location you had noticed before?	84	16
7. Give any other information which you consider of interest or value concerning your solving process.		

The percentages and the reports again parallel those of the individual *O*s. "I tried to base my solution on some probable numerical sequence, as some multiple of two. This seemed to work, but failed in the last instance. Hence my solution was incorrect." "First I tried to find a difference from the others in the box containing the cotton. When this proved futile, I tried to notice whether the

number of the box had anything to do with the number of empty boxes. At the end I thought it was the number of empty boxes minus one." "When I saw the first time that the right box was the second closed one from the right, I chose that in every succeeding trial, and, finding I was successful, did not try any other method." "My first idea was that there would be a telltale mark on the lid. I found several such marks, but the placement of the cotton did not correspond."

The procedure was obviously the same as above. Self-instructed, the *O* made one choice after another until the instruction was no longer plausible, whereupon another self-instruction dependent upon occasional instruction was formulated. This continued until either the solution was reached or the end of the presentation-period arrived. The results of this group-presentation confirm those obtained from the individual *O*s.

Problem 7. This problem was chosen to show 'insight' (= flash, new configuration?). Six matches were placed on the paper before *O*.

> *Instruction.* You have presented to you six matches. Construct of them four equilateral triangles without breaking the matches. Comment aloud.

All the *O*s went systematically to work on the presentation of the problem constructing plane figures involving triangles. This practice was interspersed with pauses where the *O* was "trying to visualize some large geometrical figure that can be broken up into four equilateral triangles," or "tried to think of all the figures involving four triangles, (some time since my geometry)." Search here was accompanied by inspection-and-comment, "Obviously each match will have to form the side of more than one triangle." "Each side of each triangle must be the length of one match." "The solution seems as if it might lie in utilizing only the half-matches but I can get only three triangles that way."

All the *O*s assumed, without any formal recognition of the fact, that the four triangles were to lie in a plane. Three of them (*Ke*, *Si*, and *My*) solved it by taking advantage of chance positions in their efforts to lay a plane figure. *Ke* made one triangle. Then he started to lay another match from the apex of it to the base. "As I was laying the match one end down and the other up, I got the idea of not having them in the same plane." *Si*, constructing plane figures, laid out two triangles with a common base so that the figure looked like a diamond with a line across the center. Taking the two matches nearest him, he lifted them, added the other, and constructed the tetrahedron. "I saw all at once that it was a tetrahedron required. Taking two of them up together gave me the notion of getting the third dimension." *My*, also constructing plane figures, started moving the matches around again and then paused with one in his hand. He started laying it down slowly, beginning with one end, "Oh, here it is," and constructed the figure.

The solution followed immediately on the chance occurrence which induced a change in the primary assumption. It undoubtedly was obtained more easily in the cases of *Ke* and *Si*, that of the former because he had taught geometry the year before and that of the latter because he was a physicist and accustomed to deal with mathematical media.

The other *Os* (with one exception) concluded it impossible to construct the required triangles. When no other arrangement could be formed under the self-instruction, the *O* sought to terminate the period. He never continued to move the matches aimlessly. On the second and third presentations he acted under a self-instructed plan; but when the instruction ceased his action ceased.

Mc never realized the impossibility of constructing the figure according to his plan. Even when shown the solution, he never saw it as a three-dimensional figure of planes in various relations to each other, but as a solid. "The idea of a solid never struck me at all. Triangle means to me something in a plane."

Results. The solution here consisted in a recognition of the three-dimensional alternative. Perceptive apprehension of a significant condition was prevented by a predisposition to lay figures in a plane. For some this set was so strong as to inhibit a recognition of the implications attendant upon lifting the matches from their two-dimensional arrangement to place them again in another plane arrangement, while for others a self-instruction that there must be something tricky or catchy about the problem created an attitude of search and uncertainty in the attempts to lay a plane figure satisfying the conditions. For these the re-apprehension of the type of figure required entailed a recognition of the predisposition to lay plane figures with the resulting formulation of another self-instruction.

Action under self-instruction, accompanied by search for a method of solution and inspection-and-comment upon the situation characterized the procedure. For those who solved the problem, the occasion brought about a re-apprehension with a consequent recasting of the instruction.

Problem 8. This problem also was intended to show alleged 'insight' (of the same sort.) The experimental object consisted of a flat steel rectangle with a slot of definite pattern cut in it. The right-left slotted groove was long and had annexed to it above and below 2 shorter T-shaped prolongations. Ten steel buttons, which could be turned or slid in any direction, were permanently fastened in the slot. Each bore a letter of the word 'perplexity,' the letters arranged in haphazard order. Because of the arrangement of the slot, the letter 'p' could not be moved from its position at the right of all the others. The solution lay in turning the puzzle and the letters on the buttons upside down with reference to the obvious top of the puzzle, which was indicated by the imposed name "The perplexing puzzle."

Instruction. Solve this puzzle, commenting aloud.

In every case the *Os* assumed at first that the word was to be spelled according to the words lying above the slot. Under self and occasional instructions, *O* shifted the buttons in every imaginable way. "It is just a matter of moving them around" (*My*).

Search and inspection-and-comment under a misleading occasional instruction characterized the early stages of the solving process. "Where has it got to be when I spell it? Where is the trouble? How do you get the Y over there? The Y is stuck" (*My*). "This takes three, and this takes three, and four have to go there, but they won't" (*Ke*). "You have to get the Y out of there first. You cannot do it. I wonder if any of these are uneven? They are all eccentric; they all roll round" (*Si*). "The Y should go to the end. I see that it is going to be the whole difficulty to get the Y out of there. The P won't let the others pass. There is something fishy here. Is there any possible way of getting them past each other? There are too many letters for this. Now what am I going to do?" (*Gl*).

At times throughout the experimental period the *O*, with an abstracted look on his face, would cease to move the buttons around, or else do so in an aimless, desultory way; but, when asked what he was doing, he reported, "I am trying to see what I can do." "I have got all but one letter in the correct positions, but I do not see how the P can be got in" (*Fe*).

Marking the later stages of the solving process were action under self and occasional instructions and elimination of one plan of action after another, shifting the buttons around until the realization was reached that the letter P could not be moved from its end position. At this point two *Os* declared its solution impossible. "I still have that P down there, and there is no way of getting it out. There must be some way for P to pass some of the others but I cannot see it" (*Ke*). "It is no use forcing the metal; that would not do any good. I cannot get that Y out of there to save me" (*Gl*). One *O* never realized this impossibility because he failed to notice the relative positions of the letters at the beginning of the observational period. "I did not remember whether the P was in that position to begin with or not. I might have got it in that place by moving the buttons around" (*Fe*).

For the others, the impossibility of the removal of the P from the end suggested utilizing that letter as the beginning of the word. "Perplexity is presumably to be spelled in this direction, but I can spell it in the other direction. May I? I can turn it upside down which is not against the directions" (*Mc*). "When I picked it up, I realized that the P was still on the end and there was no way of getting it on the other side. I must be able to spell it with P on that end, then. And it occurred to me that I could turn it upside down, twist the buttons around, and spell it forward" (*Gl*). *My* spelled it backward and turned it bottom-side up. "It was the only way it could be done. I had tried every other way of doing it and was bound to get this way sooner or later" (*My*). "The labelling would indicate that it was to be spelled like it but the P cannot be shifted" (*Si*). He spelled it backwards and then turned the puzzle upside down. "Ho, ho, wait a minute!" and he turned the buttons so that they spelled it forward, and laughed, "I turned it upside down to read it backwards, then I thought of using a mirror, and then I saw the letters could be turned around."

Results. The procedure in this problem, as in others, consisted in apprehension of the object, search for a method by which to spell the word, inspection-and-comment upon the conditions of the situation, action under self and occasional instructions, and re-apprehension due to a further realization of the limiting conditions of the experimental object.

The assumption that the word was to be spelled according to the one paralleling the slot was a misleading instruction from the occasion. Failure to arrive at the solution was due either to no change in this assumption, or to an imperfect apprehension of the experimental material, as in the case of *Fe.*

Problem 9. The following word-series were designed to bring out 'insight' of some kind. The approximate sounds of two famous Shakespearean passages—Macbeth's tragic soliloquy from the Fifth Act and the soliloquy from *Hamlet*—were presented in serial form. The Macbeth was given first because it was the less familiar.

Macbeth's Soliloquy

tomb	this	bull	lie
arrow	pet	of	Ted
and	tee	record	fools
tumor	pays	dead	the
row	from	thyme	weight
in	data	an	too
Tom	day	awl	does
are	tooth	hour	tea
oh	he	yes	death
crypt	lass	todays	
e'en	silly	have	

Hamlet's Soliloquy

two	weather	sling	oar
beer	'tis	sand	toot
knot	know	air	ache
tube	blur	row	arm
bee	inn	sough	sag
thought	them	out	ens
is	end	ray	tea
thick	twos	just	sift
west	offer	four	rub
shun	this	tune	bulls

(*a*) *Individual Os.* One of the above series was placed on a sheet of paper without the title.

Instruction. Read aloud.

The *Os* began by reading the list once. Then they invariably paused, running their eyes up and down the list while commenting. "It is a group of disconnected words. I did not think of the meanings of the words at all. Several of them are rather unusual" (*Mc*). "I noticed that there were some nicknames and a few

words together tended to make sense, and some I sought to pronounce in a different way like 'row' " (*Gl*). "I looked over it and thought of some catch in pronunciation. I thought only of each separate word. Then I started reading down fairly rapidly for myself. The 'petty pace' part gave me the first inkling" (*Si*). "I thought it might be like James's 'Pas de lieu Rhône que nous.' Then I saw 'tube' and 'bee' which looked like 'to be.' I suspected what it was then and went to reading it systematically" (*Fe*). "These words, 'tomb, arrow, and tumor,' fell together as I read them. I thought the list might be only scattered phrases, therefore I took them separately" (*Ke*). "When I read it, I knew it sounded like something faintly familiar and the first phrase and the last one made sense right off. The rest of it is this" (*My*).

The solution was dependent mainly upon the occasional instruction and only in part on self-instruction. Search is evident in the report where the *Os* ask themselves "what it can be for" or wonder "what you want done with this," or an *O* wishes that he "knew what he was to get out of it;" but in any case the *Os* knew that there was something there, not apparent but to be found.

For *Mc*, who failed to recognize the passages after several readings, the formal instruction was changed twice; first to "Read until learned," and then "Read through quickly." Under the first he got slower and slower in his reading. "They are disconnected words for groups of which I have to manufacture connections to help me learn . . . . When I tried to learn it, the slower I went, the more disjointed it became." On reading it quickly he "noticed this last time 'weather 'tis nobler.' I am beginning to get a glimmer that this is a meaningful sentence of some sort."

Gl's complete ignorance of one of the passages in question and his very imperfect acquaintance with the other prevented his getting the meaning. Time and again he struck a leading note: "A few words tended to make sense," and "I wonder if I could make sentences out of it. No, they are too disconnected." Even when the instruction was changed to "Read aloud as quickly as possible" and the report reads, "I could not help noticing the rhythm—there are places in it where a number of words almost make sense," he made no attempt to work it out as had *Ke*, (who was very unfamiliar with the passage first given him), but remained doubtful concerning it. The meaningful phrases suggested by the words were never like those in the original passage, but were like the words of the lists, *e.g.*, 'to be thought,' 'offer this sling.'

Ke, starting from the first phrase which "fell together," methodically worked out the remainder of the passage on paper, repeating phrases aloud and going back to change those already done to fit the meaning. "Two or three of these could be 'tomorrow,' and then there is 'pays from day to day to the last syllable of recorded time.' And 'whole hour yesterday.' He went over it slowly, writing down these phrases. 'Have lighted fools the way to dusty death. Tomorrow and tomorrow and tomorrow,' I see that all right." He looked at the list awhile. 'Creeps—creeps in this petty pace.' He read on slowly, 'and all our yesterdays.' Then he read from the paper the whole passage correctly.

Results. When one passage was identified, the *O* was instructed by the occasion that the other was similar. Therefore the stage of undefined search was absent in the second procedure. In general,

the *O* who recognized the first passage also recognized the other; while for the one who did not recognize the first, the second also was hard of recognition.

The disconnected and strange series of visual words tended to inhibit the recognition of the rhythmical and familiar sequence of verbal sounds.

The procedure was characterized first as perceptive and comprehensive, accompanied by a vague and diffused form of search. Occasional instruction tended to objectify concretely this element and to lead to verbal recognition. Auditory cues from a swift reading aloud led in some cases to identification with the familiar literary passage. Where the passage was not so familiar, the *O* worked out under self-instruction the meaning of the whole in a methodical way.

(*b*) *Group Os.* The series of words simulating Hamlet's soliloquy was arranged on a white cardboard 3 ft. x 2½ ft. The other series was omitted because of the lesser familiarity of the quotation.

Instruction. I have here a chart on which there is a series of words. I shall present them to you and we shall read them aloud in unison three times, increasing the speed with each reading. After the last reading you may immediately turn over your questions and answer them. Here are the words. Let's begin.

Questions	*Yes*	*No*
1. Did you wonder what it was all about when you were asked to read these words?	88	12
2. Did these words remain a group of disjointed words without any connected meaning?	53	47
3. Did you think that they might make sense before you read them?	37	63
4. Did any of the words fall together to make sense as you read them?	77	23
5. Did the whole series make sense only after the second or third reading?	27	73
6. Tell anything about your procedure that these questions have left out.		

From the percentages it is evident that about three-fourths of the *O*s failed to recognize the passage. The causes of this failure may have been either lack of familiarity with the passage or the dominance of the visual over the auditory stimuli.

The reports run parallel to those of the individual *O*s. "The increased speed in reading seemed to make them more meaningful and I left the visual words for the audible." "The unfamiliar word 'sough' was the most definite cue to me that this series was not disconnected." "There seemed enough verbs, nouns, and adjectives to make complete sentences, but there was not time enough." "I thought it was an association-experiment and I tried to memorize the words but only a few of the unusual ones stayed." "Certain words went together like 'thick, west, shun,' and 'toot ache' that could have been 'toothache.' The whole did not make sense."

The dependence of the solution on occasional instruction is quite obvious from these reports where occasional instruction (*e.g.* that it was an association-experiment) often inhibited useful cues. This may be added to those given above as another cause for failure to apprehend the words as familiar and well-known.

Problem 10. This problem was designed to show whether some form of 'insight' was concerned in applying a physical principle to a problematical situation. A deflated toy balloon, a 75-cc. flask, penstaff, pencils, and file were presented to the *O*.

(*a*) *Individuals.*

Instruction. Blow up this balloon and tie it inside the bottle.

Here the simplest and most obvious performance was to place the balloon in the bottle and blow on it. That was the procedure invariably followed, by all *O*s,—even by *Si*, a physicist. Failure led to the question, Why? "Now, why the deuce won't that blow up in the bottle?" (*Si*).

In the majority of cases the *O* quickly recognized the cause as air-pressure and lack of an escape for it. "I must have something to let the air out" (*Ke*). "I just thought of letting the air escape as I put the balloon in" (*Mc*). "All the air is in there and I have to get it out or the balloon won't blow up" (*Gl*). "I saw that it was air-pressure keeping it from blowing up" (*Si*). *My*, the logician, was the only one to make a correct scientific analysis. "To blow it up would have required compressing the air in the bottle but lungs are not strong enough to do that." An excellent example of employing an object as instrument out of its usual context occurred in this case. After finding that the balloon did not expand, he asked, "Can I use anything else?" He looked over the table and said, "I haven't the right machinery. Oh, yes; I am going to show you something now." He quickly unscrewed his pipe-stem from the bowl, inserted it by the side of the balloon and inflated the balloon, the air escaping through the pipe stem. He reported, "Therefore I put a tube in the neck of the bottle and then the air in the bottle escaped through the tube." The pipe-stem was apprehended no longer as a pipe-stem with its specific application but was generalized into a tube.

Fe, who never solved the difficulty, reported that "the problem never announced itself to me as physical. I knew the air had to get out but I never thought of it as a physical problem." To what extent that prevented a solution on his part is doubtful. In the commentary he formulated the question, "How do you get the air out of the bottle?" and he "does not see how the air can escape." Here is a definite recognition of the difficulty but no instrumental solution is suggested.

The other *O*s used the penstaff and pencil indiscriminately. *Ke* picked up the file and inserted it but decided it might break the balloon.

Results. The procedure consisted of action under self-instruction followed first by a period of search for the cause for failure under the first trial and then by a re-apprehension of the experimental condi-

tions with a consequent new self-instruction followed by action. As a general rule, when the cause for failure to inflate the balloon on the first trial was comprehended as due to air-pressure, the solution by instrumental application followed immediately.

Except for one case, *Fe*, formulation of the difficulty led to a consideration of the means of solution. In this case, however, imperfect comprehension of the laws of air-pressure inhibited the apprehension of a means of solution.

(*b*) *Group Os.*

Instruction. If I place this balloon in this bottle and blow, thus, no matter how hard I, or anyone else, blows, the balloon will not go up. Nevertheless, there is a way by which the balloon can be blown up in this bottle under these present conditions. If I gave you the balloon and asked you to blow it up in the bottle, how would you do it?

Questions	*Yes*	*No*
1. Did you find a way of blowing up the balloon in the bottle?	73	27
2. Did you decide that the air in the bottle prevented the balloon from blowing up?	72	28
3. Did you search for some means of allowing the air to escape?	61	39
4. Did you consider any of the objects lying on the table?	69	31
5. Did you think of some absent object as a means?	83	17
6. Did your solution consist in holding some object in the mouth of the bottle parallel to the neck in order to allow the air to escape?	47	53
7. Give any of your attempts at solving which are not covered by these questions.		

Various methods of obtaining the desired result in addition to the one in the sixth question were reported in the answers to the last question. "I thought of pushing the balloon in the bottle, thus allowing the air to escape as the balloon blew up. Of course, this entailed having a tube fastened to the neck of the balloon through which one could blow. I could not find the instrument on the table so considered the problem unsolved." "Wire wrapped round the neck of the balloon (in the neck of the bottle) to keep it smaller than the neck space and allow the air to escape as the balloon got larger." All sorts of other means were offered as impractical as, or more impracticable than, those quoted.

Others reported concerning the sixth question. "The object I should use is a corkscrew with the point away from the balloon because then the balloon could not expand around the object to shut off egress of air." This was another example of an instrument, not only used out of its usual context but assumed to have the generalized character of a tube. "I solved it nearly instantaneously due to my knowledge of physics and air-pressure." "Because of the question in the bead problem, I definitely started by looking over the table." The *O* was self-instructed here from a cue from a preceding experiment.

Results. Great variety in type of solution is evident in the results. Those *O*s who never apprehended the cause of the difficulty naturally failed entirely to reach any solution. Others, self-instructed from past experience, suggested various methods for the most part entirely

unsuited to the experimental conditions. Others apprehended the correct cause of the difficulty but spent the allotted time in search for a method of solution; while still others, apprehending the difficulty, had no trouble in immediately apprehending the best and simplest means of solution under the conditions given.

Inhibiting factors were, first, failure to apprehend the cause of the difficulty due to an imperfect acquaintance with the behavior of gases, and, secondly, self-instruction from past instrumental applications combined with failure to apprehend limiting conditions in the instruction and the experimental situation.

Problem 11. This problem was chosen as parallel to the foregoing. The *O* was presented with a bit of wood about 2 in. long, beveled convexly below and so rocking easily from end to end. The top was channeled out longitudinally with a concave bottom. Two small shot were placed in the groove which was then transparently covered. As the piece sat on its rocking bottom, the shot lay together at the centre, but separated by a wire partition. Small pockets were dug out at the outer ends of the groove. The problem was to cause the two shot to go right and left at the same time and so ride up to the two pockets. The solution consisted in twirling the piece suddenly, thus sending the shot centrifugally to the two pockets.

Instruction. Place a ball in each pocket simultaneously. Comment aloud.

All the *Os* began with the self-instruction that it was a matter of manual dexterity or skill. When that principle failed them, some assumed it a mere matter of chance, which was denied by the experimenter. As a result of that self-instruction the *O* tended to cease in his efforts at solution. "I do not see that there is anything to it but manual dexterity. I do not see anything but that it must go in by chance." (*Fe*). "I think it is a mere matter of skill. . . . It will just be chance if it falls in" (*Ke*). "Is it a trick of dexterity or is it a matter of luck?" (*My*).

Every method that might lead toward a solution was tried with the object in every possible position. Only in extreme impatience did the *O* violently and aimlessly shake the object. Every way of holding the box, of sudden striking and jerking, was tried,—a methodical procedure with a definite basis. "The best bet is to get one in a hole and hold it there while rolling the other along the glass top, but I have not succeeded in doing it yet" (*Mc*). "I am trying to keep one ball in the hole while I quietly and diligently get the other in, but I get it nearly there when the other one moves out" (*Gl*). "I have a better idea, I will put one in and then put another one in by striking the bottom sharply" (*My*).

Gl and *My* noticed the structure of the puzzle. *My*, who solved it, said emphatically, early in the experimental period, "One thing is certain, I suppose that wire in the center (partition) has something to do with it. . . . Why is it

made in a rolling fashion like that? There are no grooves in it any place, it is just semicircular. That wire must mean something—that one is to be on either side of it." *Gl*, who did not solve it, commented in the interrogative, "Why was it made like this? That wire down there is kind of funny. Why do you suppose it is there?"

That there might be some mechanical principle whereby it could be solved occurred to most *O*s. "One could set up an oscillatory motion so that the balls go up and down in opposite directions, I suppose" (*Mc*). "I do not know any principle of mechanics whereby both could be put in together" (*Gl*). *Fe*, when told that the puzzle was obtained from a physicist, commented interrogatively, "I wonder if the inclined plane has anything to do with it?" *My*, who solved the problem, reported that, "If I had a magnet, I believe I could do it. . . . I wonder if it depends on any scientific law. It looks as if the wire should keep them apart. You have to get them in a place where a single motion would put them both in at once. . . . What kind of motion would drive them in? The only thing left after thinking of all possible forces was centrifugal."

When those who desired to cease trying for the solution were told that the secret of it was centrifugal force, various comments were obtained showing that, though centrifugal force as a name was familiar, not one *O* had a clear notion of the significance of the concept. "I have heard of it but it means nothing to me" (*Fe*). "Centrifugal force—force toward the outside? I never would have thought of spinning it. I thought it was a mere matter of skill" (*Ke*). "Centrifugal force? I know what that is" (*Gl*). He got up and whirled around holding the puzzle in his outstretched hand. "All it ever meant to me was a whirling pail of water" (*Gl*). "When you spin a thing around?" (*Mc*). He took it in hand and tried to go around with it as one whirls round with a bucket of water. Even though these latter two now knew the principle of the solution, they never tried the correct way of spinning the object.

Results. The characteristic procedure here for those who came nearest the solution and for the one who attained it was action under self-instruction accompanied by search for a different and possibly more relevant method of solving, and by inspection-and-comment upon the experimental object.

In three cases the first self-instruction of manual dexterity was never changed—a self-instruction inhibiting the apprehension of relevant conditions which would necessitate a reformulation of the plan of procedure. When *O* found that skill of manipulation did not obtain the desired result, he ceased to attempt the solution. When he had no other feasible plan, he never continued to try aimlessly.

In other cases failure to solve could be attributed to the inhibiting effect of a vague and imperfect knowledge of the controlling physical principle.

Problem 12. This problem was designed to examine an 'insight' which might divine the suitable. Two series of numbers were com-

posed. The pattern of the one first presented was as follows. To all odd numbers additions were made; from all even ones, subtractions. The numbers so added and subtracted ranged from 1 to 14. After 12 first appeared, it then appeared in every 4th place. The pattern for the second was a subtraction-and-addition pattern; thus, $-3+2-1, +6-5+4. \ldots$

First Pattern		Second Pattern	
13	13	24	26
14	22	21	25
12	12	23	31
9	1	22	26
13	13	28	30
18	26	23	27
12	—	27	—
5		24	

Instruction. Here is a series of numbers with the last one lacking. Supply the correct number. Comment aloud.

The interesting thing about the first series for the *Os* was its pattern arrangement. All gave 12 as the number, basing their choice on this arrangement. The formal instruction was as meager as possible, leaving wide range for self- and occasional instructions. Not one *O* made any initial assumption that the other numbers followed necessarily from the first according to some ruling principle. All read the series first, inspecting its characteristics and generally seizing on what the occasion had to offer.

Various patterns were described but the one leading to the right number was the most obvious and the most generally found. "Beginning with the third number, every fourth number of the series is 12. Therefore the last one would be 12 to fit in the rule. I saw it was a pattern 13, 12—13, 12 all the way" (*Fe*). "I happened to notice that the numbers occurred in groups of four—that the first number of each group was 13, and the third (?) was 12" (*Ke*). "There are three groups with smaller ones separating them. I would say it was 12, just to keep up that sequence" (*Si*). *Mc* read the series and was struck by the regular rhythmical recurrence of certain numbers. "Without analyzing, one would say that 12 is the number because every fourth time it comes in. Just the fact of the rhythm alone would make one think that 12 was the number" (*Mc*).

My was partially self-instructed in the search for the number, "Do they go in pairs, I wonder?" He tried various schemes of adding and multiplying. After finding himself wrong, he re-read the whole series and announced, "According to the pattern the number should be 12." Occasional instruction, here as in the above cases, led to the correct number.

Gl, under self-instruction, commented, "I went through the series getting the difference between 13 and the other numbers. I found a constant relation between it and the third (?) number from it. I therefore judged it to be 12. I simply took 13 as a standard and worked from it."

Greater variety was shown in the second series. Two *Os* obtained the number by working out the scheme according to which it was composed, one because

he had inquired as to the way in which the former group was constructed, "Is that the same business of adding and subtracting?" (*Fe*); the other because systems of additions and subtractions were the simplest and most often used means of forming a series, "I read through the numbers and did not see anything. Then I began by taking the differences. I tried to see if that were the thing here, since most series are made up in that manner" (*Si*). Here the *O* was self-instructed to search for *E*'s method in constructing the series.

The occasion, however, furnished the basis for the other solutions. Self-instruction was less useful. "I have already decided that you are not going to use the same method you used before" (*Ke*). "I suppose you start with a number and the rest follow by rule" (*My*). Such self-instructions were wholly abortive. The pattern gave the correct number except in the case of *Mc*, who, by two pattern schemes, obtained the incorrect number 35, and in the case of *My*, who obtained the correct number but not through working out a pattern. By a process of elimination, counting the numbers and the number of times each occurred, he inferred that the only number in the twenties not occurring at all (*i.e.* 29) was correct.

Results. The progress toward solution consisted in, first, apprehension accompanied by inspection-and-comment upon the series and by a search for any cues likely to be of aid in obtaining the desired number, and, secondly, occasional instruction (in most cases), a self-instruction (in a few), under which the number was supplied.

Strong occasional instruction suppressed the formulation of self-instruction in the majority of cases.

Problem 13. This problem also was designed to explore for 'insight' discerning the suitable. The following stanzas with one word lacking were presented in succession. Care was taken that only one word, and that the author's, could be supplied. The five examples were chosen to bring out differences in literary perception of form and in available vocabulary.

(1) Yea, in the valley of Death I awoke,
Pallid and strange as a vision.
All of my sorrow is vanished as smoke—
These are the valleys.....

(2) Once where the unentered temple stood, at noon
No sun-ray pierced the dim unwindowed aisle,
And all the flooding whiteness of the moon
Could only bathe the outer.....

(3) Let us go hence; the night is now at hand;
The day is overworn, the birds all flown;
And we have reaped the crops the gods have sown;
Despair and death; deep darkness o'er the land
Broods like an owl; we cannot understand
Laughter or tears, for we have only known
Surpassing vanity: vain things alone
Have driven our perverse and aimless.....

Let us go hence, somewhither strange and cold,
To Hollow Lands where just men and unjust
Find end of labor, where's rest for the old,
Freedom to all from love and fear and lust.
Twine our torn hands! O pray the earth enfold
Our life-sick hearts and turn them into dust.

(4) The skies, they are not always raining
Nor grey the twelvemonth through;
And I shall meet good days and mirth,
And range the lovely lands of earth
With friends no worse than.....

(5) Well! wind-dispersed and vain the words will be,
Yet, Thyrsis, let me give my grief its hour
In the old haunt, and find our tree-topp'd hill!
Who, if not I, for questing here hath power?
I know the wood which hides the daffodil,
I know the Fyfield tree,
I know what white, what purple fritillaries
The grassy harvest of the river-fields,
Above by Ensham, down by Sanford, yields,
And what sedged brooks are Thames's.....

(*a*) *Individual Os.*

Instruction. Here is a verse with one word lacking. Supply the correct word, doing so aloud.

The first verse is composed of four alternately rhyming lines, one of the most common of rhyme schemes, the last rhyme lacking. A word of three syllables, the word 'Elysian,' which is fairly familiar, was to be supplied. All *Os* followed more or less the same procedure in attempting to supply the word. Some trusted to the feel they had for what was required. "One word to rhyme with 'vision.' 'Elision,' 'provision,' 'incision, 'ambition.' All right, 'Elysian.' That is the correct word" (*My*). "It seems as if there should be more than one word, 'valleys of something.' I am trying to find something to rhyme with 'vision' " (*Gl*). *Fi* read it again rhythmically, beating her foot to keep time, and supplying 'dum dee dum' in place of the word.

Other *Os* analyzed in greater detail with regard to metre, rhyme, and part of speech. "It needs three syllables, accent on the second to rhyme with 'vision' . . . I thought, 'Why should not an adjective do?' " (*Fe*). "The name may be some foreign proper name. This notion was strengthened by the fact that it goes with a plural noun with only three syllables to supply. There is a posture that it must be an adjective that is needed as I recall the poetic formula of modifier after noun" (*Je*). "The word has three syllables, the last part of a dactyl, and a trochee. 'Decision' fits the rhyme but makes no sense" (*Mc*).

One *O* (*Ke*) read the word immediately into the blank. Otherwise an attitude of search occurred. The others (whether or not the right word was obtained) invariably substituted other rhyming words which had no connection with the sense in attempting to get the correct rhyming word. 'Derision,' 'division,' 'decision,' 'incision,' 'elision,' 'collision,' 'oblivion,' 'provision,' 'ambition,' were variously employed. *Je* was prevented from supplying the correct word obviously because he continually assumed the word to end in 'ision,' identical with the ending of the word 'vision.'

Those to whom the word 'Elysian' occurred were convinced that it was correct if familiar to them. To *Si* the word occurred immediately, " 'Elysian valleys,' something like that is used in classical poetry. I am not sure of the word and I cannot spell it. It begins with a capital E, but I am not sure of the rest. I guess that would be right. . . . Yes, wasn't it the Elysian fields that Greek heroes went to after death?" To *Mc* and *Gl* the word was none too familiar. *Mc* reported, "I knew the word. 'Elysium' was the only thing that meant anything here, however. The adjectives from it simply did not emerge;" and *Gl* reported, "I knew it but I never thought of it. I never use it ordinarily, anyway."

The second verse is also a 4-line alternately rhyming verse with the last word lacking. This last word, 'peristyle,' is semi-technical and ordinarily absent from the vocabulary of common usage.

The search for the word ran parallel to that of the other verse. Words like 'domicile,' 'campanile,' 'mile,' etc., were supplied. Only in one case was this absent. *Ke* read 'peristyle' into it immediately. That the metre was harder here was indicated by the large number of one-syllable words supplied. *My* and *Gl* never realized the fact that more than one syllable was required. "It might be any noun that rhymes with 'aisle'—'style,' 'file,' 'defile,' 'mile,' 'Nile,' etc. The rhythm is so irregular that it might be any syllabled word. It ought to be 'pile,' a one-syllable word. A two-syllable word would be awful" (*My*).

Without formally stating it, *Mc* assumed that the word was at least of more than one syllable. He read 'domicile' into it. "Words like 'pile' spring up but they are too short." *Si* methodically attempted to get the metre, "I do not know whether it would be one syllable or three. I think it could get away with one. I will try 'domicile,' which is not the word for the metre. Three syllables sound better."

Mc, *Gl*, and *Si* tried to obtain the word from the meaning as well as from the rhyme. "It looks as though it would refer to the vestibule or something like that. Is this a technical word connected with a church? I am now visualizing a cathedral with various parts that might be illuminated by the sun. This term might apply to the roof or walls, or to the general outer part of the church" (*Mc*). "It would not be good poetry but you could say the 'outer tile' if it were made of tile" (*Gl*). "What is outer that is bathed by moonlight? So many words have three syllables but that is all. Minaret is not right, of course" (*Si*).

Only one *O* (*Mc*) had no acquaintance with the word. *Gl* and *Si* had heard of it but had no knowledge of its meaning. *Ke* had heard the word until it was familiar enough to read directly into the verse, but he was not clear as to its meaning. "What does 'peristyle' mean? It fits according to the rhyme and metre but I do not know what it means."

The third verse, the Italian sonnet, was selected for its definite and unchangeable rhyme-scheme and metre. Anyone knowing the sonnet form would have no trouble in analyzing the rhyme-ending and the exact number of syllables required. As a matter of fact, no one of the *Os* analyzed the metre to determine the number of syllables lacking. In reading the verse there seemed to be a feel for the correctly syllabled word, probably due to the familiarity, formally recognized or otherwise, of all *Os*, with the sonnet form.

Two *Os*, *Mc* and *My*, inferred the rhyme from the form. "Oh it is 'band.' I have written too many sonnets not to get that, a b b a, a b b a,c d, c d, c d",

(*My*). *Mc* supplied 'band' and said, "It is the sonnet form and I knew it would have one of the 'and' endings," but added that the rhythm or metre puzzled him at first. "The rhythm does not seem to be any good. This tended to upset the rhyme-scheme."

The others without recognizing the sonnet form gave some reason for their choice of a rhyme. "I knew it had to rhyme with 'understand' because before it had been 'hand,' 'flown' 'sown,' 'land,' and 'understand,' 'known,' 'alone;' must be 'band.' It naturally falls into first and last, second and third rhymes. I divided the thing into quatrains, and 'band' just came like that" (*Ke*).

Ke and *My* supplied the proper word from the requirements of the rhyme and metre. The sense was not considered except in so far as it did not contradict the word supplied. The others obtained the word through a consideration of the meaning. "I looked then at the sense and 'band' came" (*Mc*). "Oh, 'us,' that makes it plural. 'Have driven our perverse and aimless band' " (*Gl*). " 'Band' is the most reasonable word. 'Land' and 'hand' do not fit so well. A land is not driven generally. The 'us' is not just an editorial 'us' because it says 'our life' and 'sick hearts' " (*Si*).

The fourth verse was chosen for its irregularity of rhyme-scheme. All *Os* read the word 'you' into it, more from a sense of the demands of the verse as a whole than from any one variable, such as the meaning, the determination of the metre, or the feel for the rhyme. This was obvious in the comment of *Ke*, "I do not know why I rhymed it with 'through.' 'You' just came like that and it just happened to rhyme with 'through.' I did not definitely try to rhyme it with anything." "It is the sort of poem addressed to someone, 'friends no worse than some person' and it has to rhyme with 'through' " (*Si*). "It would not look well rhyming with 'earth;' besides, this is the usual way it rhymes. It means something to rhyme it with 'through' and it did not mean anything when I sought for a rhyme for 'raining' " (*My*).

The rhyme played a major part in the search of *Mc* for the correct word. "When I came to the third line I noticed 'mirth' and thought of 'birth' but when I met 'earth' I knew it would not do. It was a closed couplet. Also it would not rhyme with 'raining.' 'You' came and fitted in the sense." Here a very cogent reason was given for not having a consecutive three-line rhyme, but no reason at all was set forth for choosing a rhyme for the second line rather than the first. An aesthetic element, a feeling for what is most pleasing, seemed to enter here; for instance, "I do not know why I think it has to rhyme with 'through,' but it sounds better and brings the poem to a close" (*Si*).

In the last verse the meaning played the leading rôle in obtaining the absent word. "The sense decided me in favor of the 'fritillaries' rhyme" (*Ke*). "The brooks suggested Thames's 'tributaries' " (*Gl*). "I tried to do it by the sound but could not do so. Then I took the sense and sought for the relation of brooks to the Thames" (*Si*). "It filled up the sense of the sentence and rhymed with the unusual word 'fritillaries' " (*My*).

Although the meaning was the surest means of obtaining the correct word, the rhyme was given a fair amount of attention. There was very little uniformity among *Os* on this score, although all of them supplied the correct word. Two of them attempted to rhyme the word otherwise than with 'fritillaries.' "I did not know when I first read it whether the word to supply would rhyme with

'trees' or with 'fritillaries.' . . . I knew it rhymed with that word because no one would use a word like that unless he were using it for a rhyme" (*My*).

Mc was the only one to differ markedly from the other *Os*. Whereas they laid the major emphasis on meaning and only minor emphasis on the rhyme, with none whatsoever on metre, he gave prime importance to the consideration of rhyme-scheme, secondarily considering the metre, and, as a last resort, the meaning. "As I read it, it flashed through that it might be a sonnet. Then the line ending with 'tree' showed it not to be that. 'Tree' rhymed with 'be.' All the other lines are complete couplets and 'fritillaries' stood out alone without a partner." Then he read the line ending in 'fritillaries,' rhythmically beating his fingers on the table. " 'Fritillaries,' two trochees." He read the last line also moving his fingers to the rhythm, "Two trochees there, too. 'Tributaries.' I was not sure of the exact word required. What could the Thames have that I might know? 'Capillaries' came but it was not the right one. Then 'tributaries' came in with a click, not logically. I was too much concerned about the structure of the word and too little about the meaning."

Results. Apprehension of the material was followed by a period of search which varied from a general diffused type, where the feeling for what was suitable to the verse as a whole was considered, to a more concretely objectified search as requirements relative to rhyme-scheme, metre, meaning, and part of speech were analyzed and commented upon. Inspection-and-comment was a function much used.

Lack of vocabulary or a faulty apprehension of the literary form were inhibiting factors.

(*b*) *Group Os.* Copies of the first and fourth examples were presented to members of the groups.

Instruction. Here is a verse with one word lacking. Supply the correct word.

Questions (1st selection)	*Yes*	*No*
1. Are you familiar with the word 'Elysian'?	47	53
2. Did you read the verse as if you were scanning it?	69	31
3. Did you count the number of syllables required?	45	55
4. Did you try to rhyme the word with 'vision'?	96	4
5. Did you try to rhyme it with any other word?	15	85
6. Did the meaning suggest the word?	35	65
7. Did you supply the word and then test it by the meaning?	24	76
8. Did you name words ending in the 'ision' sound which fitted the rhyme but not the meaning in your search for the proper word?	80	20
9. Were you sure of the word which you supplied?	29	
10. Was this certainty due to the fitness or suitability of the word?	28	
11. Make other comments on how you went about supplying the word.		

Twenty-nine percent of the *Os* supplied the correct word, 38% left it blank, while 43% supplied various words with a more or less correct rhyme but, for the most part, lacking in meaning. These words were identical to a great extent with the words named over in the search for the proper word by the individual

Os; for example 'decision,' 'oblivion,' 'elision.' On the other hand, some of the words supplied were imperfect as to rhyme, but the meaning suited the context. Such were 'Stygian,' 'Elysium,' 'eternal,' 'arisen.' The *O*, when he did supply a word, based it on his apprehension of what was required and his inspection of the conditions. It is interesting to note that, whereas 72% of them supplied a word, only the 29% who gave 'Elysian' reported a certainty concerning the word supplied.

The reports from the last question parallel reports from the individual *Os*. "Meaning, rhyme, and number of syllables left no doubt in my mind that 'Elysian' was the correct word." "I could think only of 'eternal,' but I knew it would not fit. I tried to get a rhyme with 'vision.'" "I finally tried 'Stygian' from the river Styx. I did not like it, but could think of no other." "I took all the letters of the alphabet and tried putting them in front of 'ision'." "I knew it needed an adjective, but I could not think of any that fitted. I did not know the word anyway."

Questions (*4th selection*)	*Yes*	*No*
1. Did you read 'you' into the blank immediately?	78	22
2. Did you try to rhyme the word with any other line than the one ending in 'through'?	26	74
3. In trying to get the word, did you name over other words with the dipthhong 'oo' sound?	25	75
4. Did you determine the number of syllables required?	47	53
5. Did the meaning suggest the word?	74	26
6. Did you supply the word and test it by the meaning?	17	83
7. Were you sure of the word you supplied?	71	29
8. Was this sureness due to the suitability of the word as regards rhyme and meaning?	72	28
9. Report anything about your solving process that these questions have omitted.		

Ninety-one percent of the *Os* supplied the word 'you' in the blank; 2% supplied other words obviously unsuited to anything except, perhaps, the rhyme; and 7% left it blank.

It is evident from the reports that the *Os* were self-instructed from cues gleaned from the first verse and the questions concerning it (occasion). "My first step was to look for the rhyme-scheme. There were three possibilities, though only two (rhyme with first or second line) were likely. Then I read the verse for the meaning and 'you' came at once. It was unnecessary to count the syllables. The metre was too obvious." "The solution was very simple. After reading the verse, the word 'you' followed immediately to complete the meaning. Then I tested it for metre and found that it fitted."

The majority read the word directly into the blank. "There was no conscious effort to think about what word was necessary. There was no recognition of the rhyme. It merely was the immediate cognizance that 'you' would rhyme and fit without any other qualification." "I tried to supply a word that coincided with the spirit of the lines." "A decided sense of satisfaction prevailed throughout the process because of the felt fitness of the word immediately supplied at the first reading."

The uncertainty attendant upon some who supplied the correct word was explained as follows: "The word 'you' did not seem to fit my idea of the sense

I would like the verse to have, so I was not sure of it." " 'No worse than you' sounds quite sarcastic. The word was evidently 'you.' However, I could not see the meaning when used with 'worse'."

The procedure here consisted in an apprehension of the meaning of the verses accompanied by search for the word most suitable to the rhyme and meaning or to the stanza as a whole. Individual variations occurred in the bases given for supplying the word Some, sought for a word to fit the meaning first, others to fit the. rhyme while still others supplied it from both rhyme and meaning, or from a sense of fitness to the whole. Lack of an apprehension of the poetic form was an inhibiting factor. To some the occasion was a poetic construction; to others, a non-aesthetic verbal exercise. A wide range of occasional instructions appeared.

Interpretations and Conclusions

Throughout the course of the experiments we have constantly looked for 'insight' under its various alleged forms. In the first three problems, which encouraged 'insightful' discernment and recognition of available materials (beads and cork-in-bottle), we turned up various forms of the apprehensive functions operating to recognize tools and instruments in an unusual setting. There occurred two approaches toward solution, one involving the perceived applicability of the available instruments to the occasion offered, and the other the applicability of an instrument after reaching an imaginative solution of the problem set by the formal instruction. The first approach involved search together with perceptive apprehension, both extended by inspection-and-comment. All these psychological functions were induced and guided by occasional and self-imposed instructions as well as by a rehearsal of the problem as formally proposed. In those instances (both individual *O*s and group *O*s) where search led to an imaginative apprehension which devised promising instrumental means of solution, the actual application of these means was either itself imaginational (as in the non-executing groups) or actional (as when the individual, having once conjured up means, turned to the table and demonstrated his solution). In both cases, the protocols instanced under Problems 1, 2 and 3 will reveal the very large part usually played by verbal comment in *O*'s progress toward the solution. Just what takes the place of this comment where children, apes, and rats are wanting in linguistic resources is too often covered up by the ambiguous term 'insight.'

These first three problems also reveal some special procedures which are likewise often disposed of in the same way. We may cite the *O* who, after handling the beads of Problem 1, laid them down, leaned back, thus bringing the pliers within his range of vision, and apprehended them as means of solution. If there was 'insight' here, it was obviously an extension of the apprehensive functions *plus* inspection-and-comment. We may approach more closely the actual organic resources of solution in such a case when we observe that the enrichment of the apprehension was obviously due to the preceding search and comment, under appropriate instructions of all our three kinds, formal, self, and occasional. Having determined these essential conditions, the addition of an alleged act of 'insight' would seem to be superfluous.

In Problem 4 were the mutilated and displaced Rs, with a gradual approach from nonsense-forms to the standard letter. The formal instruction to identify each form as it came tended toward configuration, taken in the non-technical sense. The initial perception together with this instruction set up a puzzled search which led first to a family likeness among the forms as they came along, secondly to the perception of figures, animals, and the like, and finally (usually at the seventh and last member) to the symbol R. Thereupon, in retrospect, all the members had become (imperfect) Rs. The successive appearances (which were at times 'sudden') might be called 'insights'; but, more descriptively, perceptive changes under formal and occasional instructions and a puzzling search, the search gradually becoming informed and directed as the series progressed. One moment in the occasion (the prone letter) tended to block understanding (the symbol R) until the letter was practically complete in linear form. The occasional flashing out of the R would be called by some an insight; but with as little justification as the earlier perceptions.

Problem 5 called for the tracing of the rectangle and inscribed figure, with and without a central diagonal (see Fig. 2). *E* had meant to invent a problem in which trial-and-error should be supplemented by something which might be called 'insight.' As a matter of fact, the combination of occasional and formal instruction led chiefly to inspection-and-comment instead of to a headlong tracing of the lines with the red pencil. Our *O*s proved to be neither Thorndikean cats nor *Köhlersche Affen*. Upon comprehending the instructions and perceiving the figure, they set about, under inspection-and-

comment, to solve the problem. Imaginational apprehension played its part and so did search and various simple forms of action. At times a failure to solve led to a new direction of search and to a new plan. As the tracing (actual or imagined lines) proceeded, the occasion was modified, and in turn modified—by its new instructions to the organism—further procedures. This may be called trial-and-error; but it is certainly not the blundering and blind 'mechanics' of the older maze-experiments. The addition of the diagonal made the task more difficult and called out more verbal comment and self-instruction; but the general procedure was the same both with individual *O*s and in the groups. The added line decidedly changed the occasion which faced the organism and made new demands upon resources and functional expedients. Where there is an occasional 'aha!' in the protocols, it may be set down among many comments upon the course and terminus of a search. It should be noted that the 'aha!' and the 'flash' (dear to the champions of *closure*) were no more in evidence at the final solution than at various steps along the way. Here that form of the behaviorist's fallacy which interprets performance out of the experimenter's own thoughts and fancies is obviously to be discouraged.

In Problem 6 were the pill-boxes and the hidden cotton. The intent of the arrangement was to induce a situation where many possibilities offered. This intent was realized. *O* devised a plan when the boxes were presented and the formal instruction read. Upon the formulation of the plan ("I shall begin from the left," "I'll try even numbers," "I must find hidden markings," and the like), the search changed from an indefinite thrust "to solve" to "this is my way of hunting." Failure to find led to comment, to imaginative forecast, and to a new plan. As the cotton was found and replaced, the specification of method made memorial use of past success. The occasion changed, the self-instruction changed, inspection-and-comment grew; but the search went on. It is a good instance in little of scientific observation and the gradual elaboration and correction of a theory. Our commentaries throw much light upon this complex functional procedure. While many functional resources of the organism were called forth by the problem, the most notable and outstanding was comprehension, in the sense of understanding or grasping-the-significance-of. Surrogative means doubtless exist in various animals for problem-solving where plural choices offer

without perceptive cues, but our commentaries make it abundantly evident that comprehension was the central resource in all our human *O*s.

The three-dimensional construction with the matches was called for in Problem 7 and the spelling of "perplexity" in Problem 8. Both exercises are of the 'puzzling' kind. Both involve a certain spatial arrangement where something bars the way to solution. In the first it is the lay-out on the plane surface: in the second, it is the ordinary left-to-right set-up of letters in a word. Under this handicap to success, search and inspection-and-comment go on. The outcome is a manual construction. Where *O* succeeded (there were many failures), some chance change in the situation led to the match-solution, and the discovery that the initial "P" (in "perplexity") could not be moved from the right end of the slot led to the word-solution. Free trials were abundant in these puzzles; but *free trials under comment*, which tended to advance the problem toward solution. This is rather 'trial-and-advance' than 'trial-and-error.' There is nothing new here to suggest a unique operation of 'insight.' It is informing to observe how easily wont, habit, or the accustomed prevents those functional accomplishments which would lead on to invention and discovery. It appears that man's functional resources are much greater than his actual accomplishments.

In Problem 9 again (the burlesqued soliloquies) the organism was misled so far as verbal understanding was concerned. Understanding there was, from the outset, but of individual verbal symbols which did not integrate into a linguistic and aesthetic whole. If the ultimate emergence of such a whole, in a strange context, demands 'insight,' then our *O*s showed that they possessed that gift. Flashes and illuminations were absent, however; and our theories did not lead us toward 'closures.' What little clear perception there was was chiefly buried under a constantly shifted comprehension which first integrated small groups of words, then larger, and finally (in many cases), the entire Shakespearean text. Underlying these progressive comprehensions were self-instructions constantly warring with the occasional instructions and fed by a search which changed its direction when inspection-and-comment brought the hint 'this is a significant passage.' It is a fine case of two conflicting comprehensive functions, one sustained by visual receptors and the other by auditory. The visual is at first dominant; but the auditory gradually gains the ascendency by virtue of formal and self-instructions which promote

and direct search. The partial successes and the failures (in *E*'s sense!) are quite as instructive as the reports of those *O*s who read in the soliloquies. Here the groups were very informing. They reveal the organism working away under the lead of the various instructions and using one device after another. Especially do they show the effectiveness of the function *search*, a describable operation distantly related to the mechanician's 'drive.'

Problem 10 presented the flask-balloon, and Problem 11 the centrifuged shot in the rocker. Both suggested the initial comment 'puzzle to work!' which set off a fairly simple action-train (inserting the balloon, tipping and rocking the shot). Failure to carry out the formal instruction usually led to long inspection-and-comment with interspersed actions. The special resource demanded by the occasion and the formal instruction was a formulated principle or 'natural law' (air pressure, 'centrifugal force') and the comment 'This is a case of . . .' This is commonly called 'the applying of physical knowledge.' 'Knowledge' in this situation involves the *inclusion* in comprehension of a generalized rule and the *comment* that the rule applies here. The mere grasp of the principle is not enough. Understanding must fit it to the occasion. Occasional and formal instruction are not sufficient for a solution. Our commentaries and our group-questionaries show in a very striking way the functional modes involved in the solution, wherever the solution actually took place. These problems are probably more difficult for our *O*s than any preceding, in the sense that more functional devices and resources of the organism, combined in more subtle combinations, were called out. We might ignore the whole procedure and simply label the means 'insight'; but that would be to give up the central psychological problem. It is obvious that our problems of this sort only touch the surface of these important human procedures, which call loudly for searching experimental study.

In our last two problems, Problem 12 (discovering and completing a number-pattern) and Problem 13 (completing a poetic stanza), the presented occasion *contains within itself* the means for completion (*i.e.* omitted final number, omitted final word). It contains it, however, only when the occasion is *for the solving observer* a number-pattern (first problem) or a poetic structure (second problem). Here appreciation of pattern or form takes the place of outside physical principle and application in the two problems just now reviewed (10 and 11). Our commentaries and the replies to the group-questions throw a good deal of light upon the means (occasional and self-

instructions, search, comment, understanding and imaginational apprehension) which supply—and also fail to supply—the required solution. Search (for a pattern) and occasional instruction leading to comment were the chief means used in Problem 10. It is obvious that the discovery of pattern in the first series helped to define search in the second series and so led toward the goal.

In Problem 13 the temper and training of *O* were important. Without an appreciation of metrical form and of poetic phantasy, the *O*s usually found it impossible to secure the correct final word. Here we come upon the important distinction between occasion-for-the-observer and the 'stimulus' as set up or assumed by the experimenter. It is only the former, that-which-is-present-for-the-organism, that is of primary significance to the psychologist. Where the *O* poetically understood, the formal instruction set a definite search and led (through the intervention of self and occasional instruction, inspection-and-comment, and imaginational apprehension) to the missing word. A limited vocabulary sometimes defeated the search. Our group-results are of especial importance in this problem because they have helped us to discover by inquisition the relative uses of rhyme, metre, rhythm, vocabulary, poetic appreciation, and other factors, in attempting to solve the problem. Less than one-in-three of the group-members found the missing word 'Elysian' under the restrictions imposed. The fourth selection profited (91% of the group *O*s supplied 'you') from the earlier solutions, and it was also simpler and less dependent upon the appreciative abilities of the individual. Again, this experimental fragment only touches the surface of a large problem, the discovery of the psychological means resorted to by the adult and literate organism in selecting and verifying 'the suitable' in a poetic sense.

We began by listing various ways and contexts in which the term 'insight' has been recently used. Were we to review our own commentaries and the answers to our group-inquiries, we could easily find instances of the following sorts to which we might apply the word without very greatly extending it beyond its present uses. The instances are: (1) perceptive apprehension extended on the side of use and means, (2) perceptive apprehension accompanied by imaginative revaluation, (3) anticipative imagination, (4) comprehension of a solving generalization, (5) sudden drop in learning-time, (6) re-apprehension of experimental material under occasional instruction,

(7) comprehension of a general principle and its applicability in a specific instance, (8) apprehension of a total pattern-arrangement or of particular relevant features in the pattern, and (9) comprehension of a constructive scheme, of the rules or canons of an art, or of the natural relation between presented objects.

The fact is that the further experimental description and functional analysis go in the directions taken, the looser, the more ambiguous, and the less satisfactory the term 'insight' becomes. Its recent revival in psychology has been natural if not inevitable—as we saw in the beginning of our study—; but there would seem to be no longer any sanction for its varied and uncritical connotations. Under arbitrary definition it may serve for some time as a label for certain behaviors in rat-running, primate-manipulation, and school-accomplishment. As a cause and explanation of these behaviors, insight will probably be less and less appealed to in factual and experimental contexts.

Our own procedure in the problematical situation used has been (1) to present a definite and solvable problem, (2) to provide a brief, unambiguous, and constant formal instruction, (3) to employ trained and untrained observers, individually and in groups, (4) to determine by report and interrogatory[5] a descriptive account of the psychological functions brought into play, together with the instructions (formal, occasional and self) and other antecedents which threw the functions into commission.

The functions most frequently and effectively found in our solutions have been search, the three forms of apprehension (perceptive, memorial and imaginational), inspection-and-comment, and comprehension. In the examination of our descriptive material we have found no characteristic process, operation, form of conditioning or mode of discovery, which we could with propriety distinguish as 'insight.'

[5]The groups were always formed and interrogated after a given problem had been studied through the elaborate reports of trained individual observers. Only in this order could the groups be sensibly and usefully questioned, and only so could the individual and the grouped results be reasonably compared. We found this form of group-observation extremely informing.

FREE WORD ASSOCIATION AND SEX DIFFERENCE

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A familiar disadvantage of the free word association exercise for disclosing personality traits is the time and trouble involved in manipulating the data. The question to be considered here is whether a free word association exercise of the ordinary kind, in which the stimulus-word is exposed and the subject records the first associate that comes to mind, can yield equally valuable and significant results from an abridged as from an elaborate procedure. But opportunity will also be taken to summarize the sex differences in 'interest' which the association exercises used in this inquiry disclosed.

Method and Procedure

Subjects and materials. To attempt an answer to the main question the contents of two parallel free association exercises, each containing 110 stimulus-words, were scrutinized. The first exercise (Exercise A) was given to 452 *Ss*, 75 of each sex being obtained from the seventh grade, 87 from high school, 26 from college, and 35 from business. The second exercise (Exercise B) was given to other groups similarly derived, and to 510 *Ss*, in numbers respectively 80, 87, 53 and 35 of each sex.

The two lists of stimulus-words are given below in the order in which they were given to the *Ss*.

Exercise A			Exercise B		
Chair	Sixteen	Mother	Table	Wedding	Skirt
Green	Nasty	Play	Blue	Evil	Manners
Wind	Social	Toad	Anger	Artist	Filthy
Carrot	Bare	Thin	Breast	Necklace	Society
Letter	Knight	Paint	Rain	Color	Propose
True	Sneak	Lover	Turnip	Rodeo	Ring
Cellar	Strong	Darkness	Promise	Safety	Naked
Body	Memory	Art	Gentle	Religion	Castle
Despise	Mate	Electricity	Book	Handsome	Build
Ill	Fight	Save	Pure	Battle	Mouse
Face	Birth	Kill	Fame	Maiden	Cheat
Spoon	Hunt	Youth	Boss	Virtue	Athlete
Twilight	Earn	Kiss	Kitchen	Want	Love
Bashful	Hug	Soap	Flesh	Embrace	Foot
Gun	Stage	Slender	Tomboy	Wash	Feeling
Cheek	Policeman	Desire	Give	Shoulder	Fellow
Taste	Leg	Rooster	Admire	Closet	Handkerchief
Escape	Angel	Cry	Pink	Ankle	Bold
Proper	Fun	Skin	Faithful	Wish	War

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Marriage	Vain	Leader	Automobile	Duty	Baby
Diamond	Bargain	Ideal	Hair	Female	Darling
Pink	Curl	Submit	Flirt	Bell	Scream
Worship	Fear	Lily	Blush	Weep	Pain
Lovely	Mouth	Charm	Form	Picnic	Money
Honor	Dress	Clothing	Moon	Delicate	Squeeze
Longing	Curse	Rude	Modest	Waist	Clothes
Throat	Hips	Engagement	Child	Hero	Actor
Secret	Success	Bells	Bull	Fair	Protect
Sacrifice	Tender	Make	Hunt	Garden	Knee
Male	Ambition	Passion	Fresh	Physical	Devil
Party	Mistress	Arm	Snake	Serve	Enjoy
Coarse	Sissy	Garter	Stout	Power	Jealous
Brave	Gift	Dare	Fashion	Home	Shop
Flower	Immoral	Beloved	Danger	Rose	Dimple
Rule	Machine	Thrill	Powder	Magic	Afraid
Family	Shame		Sweetheart	Hand	Lips
Limb	Figure		Decent	Thirteen	

The *S*s were directed to write down 'the first word' the word shown 'makes you think of,' and the stimulus-words were exposed at 10-sec. interval.

Procedure. Since the main object of the study was to determine the most convenient and economical method of handling the data, the method of extracting sex differences was purposely varied with different groups of *S*s. First, the associations of the business group of *S*s taking Exercise A were subjected to a somewhat meticulous analysis; and then the responses of the other groups in that series (the grade, high school, and college groups) were examined afterwards, but on a slightly different plan. Then the associations of Exercise B were separately analyzed for the different groups. We shall refer to those as Analysis A′, Analysis A, and Analysis B, respectively.

Analysis A′. The responses of the 70 *S*s of the business group were treated as follows. The 110 stimulus-words were divided into five groups of which Group I contained words referring to the body and its parts, *e.g.* body, face, cheek, throat, limb, leg, curl, mouth, hips, figure, skin, arm; Group II, to sex, *e.g.* bare, beloved, birth, desire, hug, immoral, kiss, lover, longing, marriage, male, mate, mistress, passion, spoon, thrill, vain; Group III, to plants and animals, other than man, *e.g.* carrot, flower, lily, worm, rooster, toad; Group IV, to emotionally exciting words (not already classified above) *e.g.* bashful, cry, curse, fear, ill, kill, secret, shame, flight, escape, darkness; and Group V contained the remaining words, classified as miscellaneous.

It should be noted that the basis of classification was psychological, not logical—for instance, the reason for including 'spoon,' 'immoral,' and 'desire' with sex words was that the words seem provocative of thought about sex, not that their dictionary meanings are primarily sexual. Similarly the word 'ill' refers primarily or commonly to a bodily condition, but was included as emotional because illness commonly influences markedly one's emotional condition, and the thought of illness is likely therefore to have marked emotional associations. Of course a precise psychological classification is impossible—no one can be sure of striking the average mental effect of any word, and the effect varies with the individual affected. On the other hand one may easily exaggerate this difficulty —the reader will probably agree that the word groups above do constitute suffi-

ciently distinct psychological classes to justify one in attributing any marked differences between them in the kind or distribution of associates aroused to the class character.

For the purpose of this analysis single-frequency associates, *i.e.* words associated by one *S* only of the sex in question with the particular stimulus-word, were first set aside; and then the rest of the associates, *i.e.* the multiple-frequency associates, were arranged, for each in turn of the 5 groups of stimulus-words, in order of decreasing frequency of response. Thus a frequency or popularity order of associates was obtained for each sex with every stimulus-group.

The single-frequency words were also ranked for each sex in order of decreasing frequency. An associate single to a given stimulus-word with one sex might of course often occur with that sex in response to other stimulus-words, and be multiple with the other sex.

A small minority of the responses, those in which the sexes most contrasted, were studied in their association-context, that is, in relation both to the stimulus-words to which they were responses, and to 'competing' associates with the same stimulus.

In this way sex differences were elicited from the 5 different classes of stimulus-words; and, with each of these 5, for 3 different classes of responses: multiple-frequency, single-frequency, and sex-contrasting responses. Intercomparisons of the results would, it was hoped, suggest whether and under what conditions a proportion of the responses—the single-frequency, or even all but the sex-contrasting responses—might be safely ignored in deriving significant sex differences, thus preserving efficiency while saving labor.

Analysis A. For the next analysis the records, under each of 110 stimulus-words, of the words associated and the numbers of male (M) and female (F) *S*s recording each associate were arranged in separate columns for the grade, high school, and college *S*s.

The stimulus-words were divided into 5 groups as for the previous analysis. Every group was then searched successively for associates of 13 psychological classes, which had been suggested from evidence obtained elsewhere:[1] viz. words referring to (1) body parts; (2) dress; (3) personal adornment; (4) persons or people; (5) aesthetic attributes or appraisal; (6) colors; (7) activity and adventure; (8) outdoor phenomena; (9) social activity; (10) religious observance or literature; (11) highly emotional states or exciting objects; (12) economic activities; and (13) foods, culinary operations and occupations.

In determining the class of an associate it was necessary to have in mind a fairly, but not too, rigid definition of the class concerned, but there were usually doubtful cases—more so with a class like 'social' and 'emotional' than for a concrete class like 'dress' or 'colors.' Is 'complexion' psychologically a part of the body, or 'evil' a religious word, or is 'fish' to be considered a food? The same word may vary its class with the context (especially with the stimulus-word) and with the individual *S*, and the examiner may also vary on occasion in the strictness of his censorship. Examples of each class are given in Table I, to which may be added the following comments on some less self-explanatory classes.

[1]From an investigation of sex differences conducted by Professors L. M. Terman and C. C. Miles about to be published.

Two classes crossed but did not coincide; namely, the 'activity and adventure' and the 'outdoor' classes, *e.g.* 'gate post' and 'tree' are confined to the latter, 'murder, and 'fire' to the former. The 'activity and adventure' class included names of heroes and adventurous types (robber, knight, King Richard), verbs indicating physical activity or prowess (jump, hike), names of articles used adventurously (gun, aeroplane, but not automobile), dangerous conditions, daring occupations, and travel (rodeo, ship, but not hotel).

The 'social' class comprised words implying fellowship or voluntary social mixing (friend, good fun, society, but church is marginal).

The class 'religious' comprised words characteristic of sacred literature (Abraham, angels) words of religious as distinct from ethical connotation (evil, sin, holy, but not virtue, vice, or good), words for religious institutions (God, devil, Easter), and words of religious ceremony or practice (altar, hymns).

In the 'emotional' class fall words which signify emotions, or states or objects which commonly excite emotions or feelings of pleasure or unpleasure, as distinct from mere physical pain, (lonely, dearest, desire, but not wish as it is too general and weak).

Among the 'food' nouns several words (vegetable, rabbit, turkey, eggs) proved marginal cases where outdoor or animal life interests might seem to come in.

The numbers of each associate for each sex for each educational stage (grade, high school, college) were collected, and the results were tabulated separately for multiple-associates and contrasting associates, single-associates as already defined being omitted from this analysis.

It was thus possible to examine with the larger group, as with the 70 adults in the previous analysis, the relevance of different classes of stimulus-words to different classes of sex difference, and in this case in addition the relation of age or educational stage to sex differences; and to check how far with differently sized groups one might safely disregard lower multiple frequencies.

Analysis B. With the third group of 510 *Ss* (255 of each sex) the stimulus-words of Exercise B were used. The procedure was slightly amplified. As regards the classification of responses 'personal' associates were divided into 3 subclasses, for names of females, males, and persons of either sex indifferently (neutral); while to the 5 classes of stimulus-words was added a sixth, 'names of persons'; and the miscellaneous or unclassified residue (55 stimulus-words) were subdivided according as they were nouns, verbs, or adjectives. There were thus 8 instead of 5 classes of stimulus-words: 5 psychological and 3 grammatical. It should be noted as regards sex-contrasting responses that in the other analyses a response was considered sex-contrasting when the response frequencies of the two sexes differed by 3 or more. In the present analysis this basis was retained for the two smaller groups of 52 and 70 *Ss* respectively, but for the grade group of 160 a sex divergence score of 4 upwards, and for the high school group of 174 of 5 upwards was required to constitute sex contrast.

Results

Sex trends disclosed. Before proceeding to our main inquiry we summarize the sex differences in association trends gathered from

the results of the three analyses. In Table I, which follows, Exercise A was given to 35 business adults of each sex (Analysis A′), and to 191 academic *S*s of each sex (Analysis A), and Exercise B to 255 *S*s, business and academic, of each sex (Analysis B).

TABLE I

SHOWING SEX PREFERENCE OF DIFFERENT GROUPS FOR VARIOUS CLASSES OF ASSOCIATE WORDS

Associate class	Analysis	7th Grade	High School	College	Business*	Examples
(1) Body parts	A	N	F	M	FF	neck, limb, nerve,
	B	F	F	F	M	hand, teeth, hair, face, body, flesh.
(2) Dress	A	F	N	M	FF	garter, corset,
	B	N(M)	N	N	N	hose, suit, hat, stocking, shoe, coat.
(3) Personal adornment	A	N	N	N	FF	rouge, marcel, jewelry,
	B	N	N(F)	N	N(F)	diamond, ring, locket, gems.
(4) Persons or people	A	M	M	F	N	soldier, actor, hero,
	B	M	M	N	N(M)	God, hunter, silly kid, lady, artist, him.
(a) female	B	MM	M	M	M	girl, woman, sister, Mary.
(b) male	B	M	M	F	N	boy, man, father, husband.
(c) neuter	B	N	M	N(F)	F	people, children, baby, friend, teacher.
(5) Aesthetic	A	FF	F	N	N	pretty, beauty,
	B	F	F	N(F)	F	graceful, piano, picture, music, art, painting.
(6) Color	A	FF	F	N(M)	N(F)	red, white, golden,
	B	FF	FF	F	N(M)	rosy, color, black.
(7) Activity and adventure	A	MM	MM	N	M	dive, war, kill, hunt,
	B	N	M	M	M	travel, rodeo, robbers, knight.
(8) Out-door	A	M	M	M	M	auto, snake, snow,
	B	M	M	N(M)	M	deer, grass, coach, war, ride, woods.
(9) Social	A	N	N(F)	N(F)	N(M)	friend, companion,
	B	N	N(M)	N	N	party, picnic, dances, play, fun.
(10) Religious	A	N	N(M)	N	N	Jesus, God, church,
	B	N	N	N	N(M)	devil, saint, holy, Bible, righteous.
(11) Emotional	A	FF	FF	M	N	ecstasy, good time, admire, happiness, fear,
	B	N(F)	F	N(F)	N(M)	anger, longing.
(12) Economic	A	M	M	N	N	money, cheap, work,
	B	N	N	N	N	counter, wages, spend, dollars, earn, sale.
(13) Foods	A	M	MM	F	N	eats, coffee, soup,
	B	M	MM	N	N	chicken, ice cream, sugar.

*Analysis A′ for this group.

The letters 'M' and 'MM' or 'F' and 'FF' signify distinctive, and markedly distinctive, male or female tendencies. Parentheses signify that the predominance is probably non-significant, and 'N' that there is no distinctive sex tendency.

The differences in sex preferences for different classes of associate words are summarized in Table II. This summary shows that though

TABLE II

SUMMARISING SEX PREFERENCE OF THE EIGHT GROUPS FOR DIFFERENT CLASSES OF ASSOCIATE WORDS

	Associate Class	Sex Trends				
		FF	F	MM	M	N
(1)	Body parts	1	4	0	2	1
(2)	Dress	1	1	0	1	5
(3)	Personal adornment	1	0	0	0	7
(4)	Persons or people	0	1	0	4	3
(a)	females	0	0	1	3	0
(b)	males	0	1	0	2	3
(5)	Aesthetic	1	4	0	0	3
(6)	Color	3	2	0	0	3
(7)	Activity and adventure	0	0	2	4	2
(8)	Outdoor	0	0	0	7	1
(9)	Social	0	0	0	0	8
(10)	Religious	0	0	0	0	8
(11)	Emotional	2	1	0	1	4
(12)	Economic	0	0	0	2	6
(13)	Foods	0	1	2	2	3

there is no class of associate or response words scoring the same positive sex trend in every group of *Ss* for both the parallel exercises, that is, scoring eight like positive signs, in classes 3, 5, 6, 7, 8, 9, 10, 12, and in 4a, the tendency is either neutral or distinctive of one sex only, of the female sex in classes 5 and 6, less emphatically in 3; of the male sex in classes 7, 8, and 4a and less emphatically in class 12 in which the 'economics' trend is too slight to be conclusive or decisive, neutral indices predominating here, as also in classes 3, 9, and 10.

The table shows the six remaining classes 1, 2, 4, 4b, 11, and 13, ambiguous—sex trends varying according to the group or exercise. But the evidence of the very small Analysis A college group (26 *Ss* of each sex) conflicted with that from the college group twice its size in 7 out of 13 cases, and was in other respects singular. For example it scored the only M for 'dress' associations, the only F

for 'personal' and 'food 'associations, and the only clear M for 'emotional' word associations. It seems fair to conclude that the group through small size or otherwise was unrepresentative; and we may take the bulk of the evidence here as indicating a female preference for dress, and parts of the body, and emotional associates.

Personal associates score M in 4 groups, F in 1, N in 3. But in a subsequent subdivision (in Analysis B) into words for males, females, and of either sex indifferently, it appeared that associating females is a distinctive male tendency in every group, there being no uniform male preference for the other two classes of persons; nor have females a general preference for names of males.

Moreover, in connection with Analysis A′ the personal associates with 26 sex stimulus-words, if further subdivided, show as follows: number of males recording feminine nouns, 73, masculine nouns, 24; number of females recording feminine nouns 22, masculine nouns 42.

This means a very marked male preference for feminine nouns and a less marked female preference for masculine nouns; and suggests that thinking of the 'opposite' sex is stronger in males than in females, and that not only at the school stage. It also illustrates the risk of accepting sex agreements or differences for large classes of objects in general without further analysis. An aggregate male score for personal names may conceal a special tendency to think about girls or women as objects of distinctive sex attraction.[2]

The same need of deeper analysis, if we are to get at the effective sex motives, is illustrated by the 'food' word associates. These score M or MM in 4 groups, F in 1, N in 3. But, as our discussion of sex differences in relation to stage will presently suggest, this need not indicate any particularly general distinctive male interest in food.

Distinctive female associate preferences appear most marked for aesthetic and color words, less marked for words for parts of the body, for articles of personal adornment, and emotional words; males distinctively associate outdoor words and words of activity and adventure, names of females, and perhaps (but see below) words for foods. Over economic, social, and religious words there is no very noticeable sex distinction, nor, in curious contrast with other evidence, for words for articles of dress.

[2]Further segregation showed that aunts, grandmothers, and sisters were not associated more often by males than brothers, uncles, and grandfathers by females, or by males either.

Associates in terms of politics, history, geography, science, and mechanical or manual occupations, occurred too rarely to warrant classification or generalization.

Correspondence in results from Exercises A and B. Table I shows that out of 13 classes of associates or thought-referents for which the sex scores in the two parallel series A and B are tabulated, for each of 4 groups of *Ss*, yielding 52 cases for comparison, in three cases the sex trends of the two series are opposed, in 36 the same, and in 13 definite in one series and neutral in the other. In the 26 cases where the two larger groups are concerned (7th grade with 78 and high school with 87 *Ss* of each sex) there are no opposite, and there are 22 similar, trends and 4 partially discrepant (neutral versus definite).

In the 26 smaller group cases (college groups with 26 and 53 *Ss* and a business group with 35 *Ss* of each sex) there are three opposite and 14 similar trends and 9 partially discrepant. Of these smaller groups the 26/26[3] college group, and perhaps the 35/35 business group in series 2, seem to be least representative, if consonance with a majority trend of the groups in its series be any criterion. The smaller college group trend contrasts with the majority trend in its series in 6 cases, but the larger 53/53 college group in none. Decrease of reliability with decrease in size of group is what we should expect. The evidence of college group B is thus probably the more trustworthy for adults.

Sex differences in relation to stage and age. The doubtful trustworthiness of the smaller groups of older *Ss* makes stagewise comparisons where they are concerned precarious. Taking the scores as they stand the extent of correspondence between stages is summarized in Tables III and IV.

Age and stage preferences disclosed. Summarily we may say that the two school groups concur in their sex trends on the one hand, and the two adult groups on the other, more closely than do adult with school *Ss* (Table IV); that sex differences in religious and social associates are negligible throughout (Table I); there is a rather general distinctive male interest in female persons but not in personal associates as a whole; and there is a rather general female preference for aesthetic associates (Table I).

As regards particular agreements and disagreements the following points may be made: (1) A female tendency towards aesthetic

[3]The first number is of male *Ss*, the second of female.

expression seems to characterize the youngest stage, and towards emotional associates and color words the school stage, in general (Table II). (2) A male preference for male person associates, which distinguishes the grade from the college groups, and a more marked male preference at the grade, but especially at the high or adolescent stage, for words of activity and adventure (Table II) is perhaps attributable to the youthful male's heroic and 'he-man' predilections.

TABLE III

SHOWING AGREEMENT AND DISAGREEMENT BETWEEN GROUPS IN SEX TREND

Group		7th Grade	High School	College
High School	Concurrence	25		
	Opposition	0		
	One N, the other M or F	4		
College	Concurrence	13	13	
	Opposition	5	5	
	One N, the other M or F	11	11	
Business	Concurrence	13	14	16
	Opposition	1	4	3
	One N, the other M or F	15	11	10

The difference among the various groups, in both exercises, is shown in Table IV.

TABLE IV

SHOWING ASSOCIATE-WORD CLASSES IN WHICH GROUPS DISAGREE IN BOTH EXERCISES

Groups	Associative classes differing
Two School Groups	No cases
Grade and College	Persons, Aesthetic, Activity, Foods
High School and College	Persons, Aesthetic
Grade and Business	Persons, Body Parts
High School and Business	Persons, Colors, Emotional, Foods
College and Business	No cases

It is noticeable that the business groups score more markedly male than the college (the more intellectually interested?) groups in these respects (Table II). (3) The business group females prefer adornment and dress associates more distinctively than the college or school females. (4) A strong male preference appears for 'food' words at the school, especially the high school, stage, in contrast with the adult female or neutral tendency. The fact, however, that 'wine' scores 21 M to 1 F in association with 'cellar' in the high

school group, and that many food words are also words for outdoor plants and animals suggests that further analysis of a wider range of content is required before we can infer a distinctive male interest in foods as such. (5) Sex divergences appear more pronounced at the school stage than later. Out of 58 indices 21 are N or neutral for the school groups and 35 for the adult groups, while in 10 cases the distinctive tendency is markedly pronounced in the school scores, and in only 4 cases in the adult groups. Average sex divergence scores based on the associations considered in our analysis of exercise A were approximately 4.2, 3.4 and 2.8 for grade, high school and college groups respectively. We will now come to the main object of this study.

Longer and shorter procedures compared. (*1*) *Analysis A'.* Table V below indicates numerically for the business group the extent of correspondence in scores and signs of 4 different collections of associates, namely, single, multiple, total, and contrasting associates. Contrasting cases cover all associates in which the difference between the sexes in score is 3 or more.

According to this table, the multiple associates show the same sex predominant as the total associates in every one of 13 classes of associates, but the proportion of predominance differs a little in some cases. In other words, disregard of the single associates would not have altered the sex character in any case, but would, (though probably within chance limits) have reduced the differences in some cases. On the other hand, the single associates scores do actually show a different sex predominant (however slightly) from that indicated by the total scores in the case of personal, aesthetic, color, social, emotional, economic and food associates.

Comparing the multiple scores with those of contrasting cases we find the same sex predominant in 11 out of 13 cases, and, in 1 of the remaining 2 (personal) 'contrasting' cases show a slight male against a 'multiple' slight female predominance (111/100 to 265/278) yielding an N sign in both cases, and in the other (colors) there is a 'contrasting' N (13/13) against a 'multiple' F 35/59. But in this case the 'contrasting' numbers are too small to signify. In other words, where the contrasting case numbers are at all considerable, and in most other cases, there is agreement or near agreement in the sex character; but we cannot rely on this agreement obtaining where the score for an associate is low.

The 'contrasting' cases tend to accentuate sex divergences—in 7 cases they have double signs against 3 for the 'multiple' associates,

TABLE V

ANALYSIS A′: CORRESPONDENCE OF HIGHER WITH LOWER FREQUENCY RESULTS

	Associate class	Score and sex trend for: Multiple associates	Single associates	Total associates	Contrasting cases
(1)	Body Parts*	66/142 = FF	36/46 = FF	102/188 = FF	25/45 = FF
(2)	Dress	28/56 = FF	21/40 = FF	49/96 = FF	12/21 = FF†
(3)	Adornment	24/58 = FF	13/29 = FF†	37/87 = FF	23/51 = FF
(4)	Personal	265/278 = N(F)	190/189 = N	416/437 = N(F)	111/100 = N(M)
(5)	Aesthetic	55/74 = N(F)	52/47 = N(M)	105/110 = N(F)	7/32 = FF†
(6)	Colors	36/59 = F	19/16 = N(M)†	55/75 = N(F)	13/13 = N†
(7)	Act. & Adven.	83/51 = M	53/53 = N	136/104 = M	35/11 = MM†
(8)	Outdoor	196/167 = M	73/68 = N(M)	269/235 = M	74/59 = M
(9)	Social	46/36 = M	29/31 = N(F)	75/67 = N(M)	23/21 = N†
(10)	Religious	66/58 = N(M)	28/20 = M†	90/80 = N(M)	11/4 = M†
(11)	Emotional	156/176 = N(F)	117/112 = N(M)	277/288 = N(F)	38/66 = FF
(12)	Economic	64/80 = N(F)	34/28 = N(M)	98/108 = N(F)	15/24 = F†
(13)	Food	84/63 = N(M)	26/63 = F	110/103 = N(M)	52/36 = MM

*66 words for parts of the body were associated more than once by males, and 142 by females, and 36 once by males, and 46 once by females; and 25 words for parts of the body were associated by males on more than three occasions, and 45 similarly by females.

†Too few to signify.

TABLE VI

ANALYSIS A: STATEMENT OF SEX TRENDS IN SCORE WITH CORRESPONDING INDICES FOR 'MULTIPLE' AND 'CONTRASTING' ASSOCIATES IN 3 ACADEMIC GROUPS

Associate Class	7th Grade Associates		High School Associates	
	Multiple	Contrasting	Multiple	Contrasting
(1) Body Parts	N 425/424	N 234/229	F 559/602	F 363/397
(2) Dress	N(F) 127/138	F 98/127	N(M) 175/160	N 133/137
(3) Adornment	N 82/89	N 14/14	N 109/122	N 87/80
(4) Personal	N(M) 797/736	N(M) 492/444	M 1135/967	M 702/586
(5) Aesthetic	FF 108/180	F 71/122	F 133/162	F 77/96
(6) Colors	FF 108/163	FF 29/76	F 138/156	F 78/106
(7) Act. & Adv.	MM 377/216	MM 219/110	MM 401/298	MM 287/156
(8) Outdoors	N(M) 342/305	N(M) 212/201	N(M) 422/388	N 314/319
(9) Social	N 83/90	N 64/64	F 93/128	F 54/71
(10) Religious	N(F) 106/110	N 74/75	N(M) 126/112	N(M) 54/45
(11) Emotional	F 475/675	F 330/490	F 490/633	F 314/513
(12) Economic	N(M) 209/184	M 162/127	N(M) 202/185	N 147/140
(13) Food	N(M) 201/191	N(M) 130/124	M 231/190	M 200/166

Associate Class	College Associates		Total	
	Multiple	Contrasting	Multiple	Contrasting
(1) Body Parts	N(M) 111/92	MM 64/34	N 1095/1118	N 661/660
(2) Dress	N 29/31	N 17/13	N(F) 313/337	N(F) 237/278
(3) Adornment	N 33/39	N 3/6	N 227/240	N 104/100
(4) Personal	F 241/295	F 122/146	M(N) 2172/2008	M(N)1316/1176
(5) Aesthetic	N 43/42	N 22/21	F 284/384	F 170/240
(6) Colors	N 32/30	M 20/14	F 278/349	F 127/186
(7) Act. & Adv.	N 84/85	N 41/42	MM 872/599	MM 547/308
(8) Outdoors	M 94/78	M 30/25	M 858/770	N(M) 556/548
(9) Social	F 20/26	M 14/10	F 196/244	N(F) 132/145
(10) Religious	N(M) 42/36	N 28/29	N 274/260	N 156/152
(11) Emotional	N(M) 179/165	N(M) 59/51	F(FF) 1296/1658	F(FF) 697/1075
(12) Economic	N 49/45	N 32/37	M 460/414	M 341/304
(13) Food	F 53/65	F 28/34	M 485/446	M 358/324

the single associates, and the total figures; and only 3 neutrals, against 6, 8, and 8 respectively. The contrasting cases tend to bring into prominence the same sex divergences which the multiple and the total figures manifest. We may also add that a scrutiny of the contrasting cases in their entirety, that is, in relation to stimulus word and fellow associates, is more illuminative of the mental traits behind the sex differences than a study of the statistical differences alone.

TABLE VII

NUMBER OF 'CONTRASTING' AND 'MULTIPLE' ASSOCIATE-WORDS

Associate class	Multiple		Contrasting	
	M	F	M	F
(1) Body Parts	1095	1118	661	600
(2) Dress	313	337	237	278
(3) Adornment	227	240	104	100
(4) Personal	2172	2008	1316	1176
(5) Aesthetic	284	384	170	240
(6) Colors	278	349	127	186
(7) Act. & Adv.	872	599	547	308
(8) Outdoors	858	770	556	548
(9) Social	196	244	132	145
(10) Religious	274	260	156	152
(11) Emotional	1296	1658	697	1075
(12) Economic	460	414	341	304
(13) Foods	485	446	358	324
Total each sex	8900	9027	5203	6556
Total both sexes	17,927		10,659	

We may conclude that for this particular exercise at any rate, and presumably for exercises with similar ratios of stimulus words to subjects, we can safely neglect single associates in our analysis, thus saving labor in the preliminary tabulation. In this exercise single associates accounted, on a rough calculation, for some 36% of the total.

(2) *Analysis A.* We now come to Analysis A, in which single associates were disregarded and 'multiple' compared with 'contrasting' associates for 3 differently sized and aged groups. Table VI is relevant.

Taking all the groups and cases we find that out of 52 comparisons in 46 the multiple and contrasting cases yield the same signs, that is agree in sex difference or in absence of sex difference. With the two larger groups of *Ss* (grade and high school) in all 26 cases

there is a close or (in two) fairly close agreement; and either the same sex predominates or the departures from equality are too small to signify.

In the smallest, namely the 26/26 college group are these three cases of difference in signs:

	Multiple	Contrasting
Body parts	111/92 N(M)	64/34 MM
Colors	32/30 N	20/14 M
Social	20/26 F	14/10 M

Here we have either a sex distinction the same but (in one case only) more pronounced, or non-significantly small numbers.

In fine, where the numbers of *Ss*, of stimulus-words, and of the associates in any given case, are adequate, a scrutiny of cases of sex divergence in score of 3 or more yields the same sex distinctions as a more comprehensive survey. Specifically, with *Ss* exceeding about 70/70, and stimulus-words exceeding about 100, the briefer procedure would appear to suffice. But where the associates of any class fall below 50 in all, we may do well to supplement from more comprehensive data. No doubt exact statistical treatment would produce a more precise criterion of adequacy for different ratios of *Ss* to stimulus-words, always presuming that other cicumstances are also sufficiently similar. Table VII suggests the saving of time and trouble that would be thus effected by reducing the number of responses to be considered.

(*3*) *Analysis B.* In Analysis B we come to a second set of stimulus-words (Exercise B) analogous to that for Analyses A′ and A. In this case we compare the evidence from the total scores of the associates considered with that for the scores of contrasting cases for 4 differently sized and aged groups. But whereas in Analysis A by 'contrasting' was uniformly meant a sex divergence in score of 3 or more, in the present analysis 3 is retained for the 2 smaller groups of *Ss* (the 53/53 college and the 35/35 business); but the figure is raised to 4 or more for the grade school *Ss* (80/80), and to 5 or more for the high school *Ss* (87/87), on the assumption that the more *Ss* the higher the scores for any associate and therefore the greater the proportion of wider score divergences, so that the safety limit for contrasting cases can be correspondingly raised. Table VIII summarizes the data.

TABLE VIII

ANALYSIS B: STATEMENT OF SEX TRENDS IN SCORES WITH CORRESPONDING INDICES ATTACHED FOR ALL, AND FOR CONTRASTING, ASSOCIATES IN 3 ACADEMIC GROUPS OF SUBJECTS AND 1 BUSINESS GROUP

Associate class	7th Grade (80/80) +4 up, sex divergence in score		High School (87/87) +5 up, sex divergence in score		College (53/53) +3 up, sex divergence in score	
	All cases	Contrasting cases	All cases	Contrasting cases	All cases	Contrasting cases
(1) Body Parts	F 487/508	F 246/319	F 542/590	F 332/365	F 254/286	F 168/175
(2) Dress	N(M) 233/218	N(M) 164/140	N 267/268	N 183/161	N 164/167	N 91/97
(3) Adornment	N 91/89	N 55/67	N(F) 83/111	N(F) 42/64	N 82/88	N 29/36
(4) Persons	M 919/757	M 466/372	M 1048/849	M 574/468	N 561/559	N(F) 274/290
(a) female	MM 353/240	MM 212/134	M 442/332	M 336/242	M 297/245	M 163/119
(b) male	M 325/271	M 142/119	M 303/257	N 100/96	F 138/178	F 53/96
(c) neuter	N 243/246	N 112/119	M 304/250	M 168/123	N(F) 125/136	N(F) 58/84
(5) Aesthetic	F 199/276	F 93/156	F 266/325	F 122/164	N(F) 150/171	N(F) 78/90
(6) Colors	FF 194/251	FF 93/146	FF 211/318	FF 151/238	FF 191/251	FF 95/146
(7) Act. & Adv.	N 314/313	N 137/146	N 337/276	M 154/113	M 181/149	N(M) 84/68
(8) Outdoors	M 470/422	M 247/219	M 479/437	N(M) 211/202	N(M) 292/284	N(M) 161/154
(9) Social	N 123/118	N 60/60	N(M) 133/118	N(M) 34/29	N 56/52	N(M) 27/19
(10) Religious	N 88/83	N 0/0	N 121/114	N 30/35	N 61/72	N 45/48
(11) Emotional	N(F) 577/657	F 197/262	F 617/738	F 208/312	N(F) 405/446	N(F) 246/261
(12) Economic	N 140/141	N(F) 64/79	N 165/162	N(F) 68/82	N 89/70	N(M) 48/32
(13) Foods	M 221/161	M 106/66	MM 214/142	MM 100/40	N 119/117	N(F) 34/40

TABLE VIII (Continued)

ANALYSIS B: STATEMENT OF SEX TRENDS IN SCORES WITH CORRESPONDING INDICES ATTACHED FOR ALL, AND FOR CONTRASTING, ASSOCIATES IN 3 ACADEMIC GROUPS OF SUBJECTS AND 1 BUSINESS GROUP

	Business (35/35) +3 up sex divergence in score		Total (255/255)	
Associate class	All cases	Contrasting cases	All cases	Contrasting cases
(1) Body Parts	M 155/98	MM 83/42	N 1438/1472	N(F) 829/901
(2) Dress	N 100/97	N 65/59	N 764/750	N(M) 503/457
(3) Adornment	N(F) 37/50	N(F) 19/28	N(F) 293/338	N(F) 145/195
(4) Persons	N(M) 348/322	N(M) 123/101	N(M)2876/2487	N(M)1456/1233
(a) female	M 165/122	MM 63/28	M 1257/939	M 764/523
(b) male	N 109/105	N(M) 37/29	N(M) 872/811	N 332/240
(c) neuter	F 74/95	FF 22/43	N(M) 746/727	N 360/369
(5) Aesthetic	F 77/112	N 32/33	F 592/894	F 325/443
(6) Colors	N(M) 118/97	M 29/21	F 714/917	F 368/548
(7) Act. & Adv.	M 92/72	M 19/10	N(M) 924/810	N(M) 394/337
(8) Outdoors	M 166/135	M 66/43	N(M)1407/1278	N(M) 685/618
(9) Social	N 43/42	MM 14/3	N(M) 355/330	N(M) 135/111
(10) Religious	M 52/38	MM 18/4	N(M) 322/307	N(M) 93/87
(11) Emotional	N(M) 220/192	N(M) 89/80	N(F) 1819/2033	F 740/921
(12) Economic	N 64/58	M 32/23	N(M) 458/431	N 212/216
(13) Foods	N 73/79	N(F) 22/36	M 627/499	M 262/191

Taking all groups and cases we find that out of 64 comparisons in 56 the total and contrasting cases yield the same signs, that is the same noticeable sex divergence or absence of sex divergence in tendency to associate or think of, the class of word or object in question. In the remaining 8 cases an N sign is attached to one of the 2, the total and the contrasting, sets of scores and a definite sign to the other; but in all 8 cases the total and contrasting associates differ not in the sex character of tendency evidenced but just enough in degree to admit our distinguishing one as N and the other as of a definite sex character. Moreover, in 4 of these cases of partial discrepancy the scores for the contrasting associates were precariously low. Wherever the score is adequate the same sex trend is indicated, but occasionally less markedly by one set of associates than by the other.

The results of our analysis suggest strongly that, for an exercise in which the stimulus-words and the *S*s both exceed 100, for all practical purposes every associate for any given stimulus-word in which the sex scores differ by less than 3 can be safely disregarded; and that where the *S*s number 160 or more we may raise that limit to 4 and even 5. This applies to the 13 psychological categories or directions of interest which a scrutiny of the associates, and other evidence not considered here, suggest as most likely to differentiate the sexes in the present experiment. But the proviso should be added that where the associates of any category receive less than a certain minimum score (in ordinary cases 50 would seem to serve) a reduction in the contrasting limit would be on the safe side.

Relation of class of stimulus-word to class of associate. That the exercise under survey fails to elicit in adequate numbers certain presumptively important classes of associates has already been noticed: stimulus-words of the kind to elicit these were absent from the series. That the stimulus-word, as one would expect, influences the associations, and stimulus-words of one class predispose to corresponding classes of associates (corresponding is not necessarily similar to the stimulus-word), and what some of these 'correspondent' classes are, our present analyses indicate. Comparison of responses in relation to stimulus-words shows that words for activity and adventure are elicited most from emotional word stimuli, not from sex words or from words for the parts of the body; words for personal adornment from sex not from plant or animal word stimuli; outdoor words from plant and animal stimuli not from emotional; personal

names from sex and personal stimuli; and from sex stimuli feminine noun associates result far more often than masculine or neutral nouns; color associates are fairly evenly distributed, but are evoked most especially from plant and animal stimuli; social words issue from emotional and personal stimuli, not from sex, body, or plant and animal—and so on. That these predominances are so pronounced indicates one of the difficulties with exercises of this type: the associations are so far determined by the stimulus-words that little is left to separate the sexes; so that with any given class of stimulus-words community of associations between the sexes far exceeds diversity. Divergence scores are an inconsiderable percentage of the total gross scores. The most marked general sex divergence, for example, in Analysis A is in emotional associates, which total 1296 for males and 1658 for females; that is, a divergence in amount of 13% only on the total. As one works through the gross figures likeness is far more impressive than unlikeness, even with stimulus-words already, as in the series under scrutiny, selected out of others for their sex-differentiating capacity.

Conclusions

Having now considered the free associates of 4 different groups with 2 series of stimulus-words and studied for each the sex differences in frequency of response along 12 to 15 lines of interest, we conclude from analysis of one group's responses that single associates (words associated once with a given stimulus-word) may be safely disregarded without significantly affecting the results; and, from two analyses of the responses of 4 groups with two sets of stimuli, that we may limit our consideration to 'contrasting' cases and obtain, with a margin for chance deviations, the same sex distinctions as where we consider either all, or all but the single, associates. This conclusion holds provided (a) the 'contrast' spans are adjusted to numbers of *S*s and stimulus-words; and (b) where a topic of interest yields fewer associates, recourse is had to multiple or total responses.

A word is required on both these conditions. Regarding (a), the spans suitable for sample cases have already been suggested (*supra* 467). If the scores and, what is also important, the number of instances which contribute to those scores, are to be enough to signify, we need an adequate number both of stimulus-words and of *S*s. The more the *S*s or the more the stimulus-words the more numerous the instances expectable of spans of any given width;

hence to insure a significantly high aggregate score and a sufficient number of instances we may raise or lower the span of contrast correspondingly. It remains for the statistician to attain precision. As regards (b) a difficulty with free associations is that we can not tell beforehand the fertility of associates along any line of interest, though a repeated experiment would, no doubt, *coeteris paribus*, give statistical probabilities; so that we cannot exclude the necessity on occasion of supplementing our contrasting cases with a study of multiples. Moreover, as contrasting cases are a fraction of the whole, it sometimes happens that a single stimulus-word may yield a single associate with a high score and a wide sex divergence, (*e.g.* the word 'wine' as one of the food associates), and misleadingly bulge the aggregate sex scores. Hence not just a sufficiently high aggregate score, but a sufficient number of cases, and an eye open for unevennesses of this kind, are necessary precautions in the use of the contrasting case method. It cannot be applied, nor indeed can any method, quite mechanically.

Moreover, it appears that (1) as the sex differences disclosed depend upon the class of stimulus-word, some may easily escape representation (*e.g.* associates in terms of social or physical sciences, mechanical or manual occupations escaped the exercise under examination); and (2) the sexes agree far more than they differ in their responses. In fine a wider range of stimulus-words with a shortened analysis, confined to the most sex-contrasting responses, is more informative than a full analysis with a narrower range, and is the more generally practicable. In using free word associations, therefore, fully representative stimulus-words with examination of the most divergent associates is, it appears, the economical program.

SYMMETRY IN VISUAL RECALL

By F. Theodore Perkins, University of Kansas

Most of the experiments on memory, even the classical ones of Ebbinghaus,[1] are chiefly concerned, not with memory, but with learning. Even the *Aussage* experiments initiated by Stern,[2] which minimize the aspect of learning, were not entirely free from the complications of the learning problem. Few studies have been solely concerned with the problem of memory. The list is short.

Hempstead,[3] in 1901, noted in a tachistoscopic experiment that the changes in memory seemed to be "guided by the principles of symmetry and similiarity."[4] Kuhlmann,[5] in 1906, found that the reproductions of meaningless visual forms, which were made from 2–90 days after presentation, tend toward standardization: slight differences are omitted and parts are standardized, equated, and symmetrically arranged. Wulf,[6] in 1922, found that successive reproductions of a visual figure change continuously and gradually toward greater symmetry, and suggested a theory based upon laws of balance and stability of energy patterns. Lastly, Gibson[7] repeated Wulf's experiments in modified form, and concluded, contrary to Wulf, that the changes toward symmetry were to be explained by habit.

In the methods of both Wulf and Gibson there are certain objections to be raised in order to satisfy the requirements for a critical investigation of the memory process. First, the material should be presented only once, a stipulation that is violated by Gibson and to some extent by Wulf in his use of the 'part-stimulus.' Secondly, the stimulus-figures should be simple, in order that one brief presentation will suffice. Thirdly, there should only be a few stimulus-figures, in order that the average observer will be able to reproduce all of them after the first presentation. Fourthly, the material should be reproduced entirely from memory at various time-intervals to make possible a study of the trend, direction, and extent of the changes with succeeding increments of time, thus giving a temporal picture of the memory process.

*Accepted for publication March 19, 1931.

[1] Hermann Ebbinghaus, *Über das Gedächtnis*, 1885, 1-114.

[2] Wm. Stern, *Beiträge zur Psychologie der Aussage*, Leipsic, 1903-6.

[3] L. Hempstead, The perception of visual form, this Journal, 12, 1901, 185-192.

[4] *Ibid.*, 192.

[5] Fred Kuhlmann, On the analysis of the memory consciousness: A study of mental imagery and memory of meaningless visual forms, *Psychol. Rev.*, 13, 1906, 316-348.

[6] Friedrich Wulf, Beiträge zur Psychologie der Gestalt: VI. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

[7] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

Problem

The present experiment was conducted for the purpose of ascertaining the nature of the changes that occur in the recall of visually perceived forms.[8] The problem, although the same as that of Wulf and Gibson, differed in the material and in the method used. The experiment was divided into two parts designated as A and B, on the basis of the two sets of materials used.

Method

As the method was the same for both parts, it will be described only once. Two sets of 5 simple, nonsymmetrical, geometrical figures were drawn with black ink on cards 14 x 11 in. The first set, called Cards a (see Fig. 1), was used with the A group of *O*s, while the second set, Cards b (see Fig. 1), was used with the

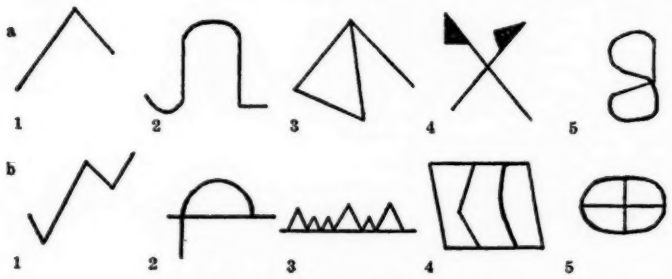

Fig. 1. Stimulus Figures

B group. In all there were 150 *O*s, 98 of whom made up Group A, and 52, Group B. The *O*s were all naïve with respect to the problem as well as to psychology in general. At no time were the results of the experiment disclosed to them, or did they know in what way the research was related to the problem of memory. They were all students in beginning psychology laboratory and the experiments were performed during the regular class period. The series of figures was presented to groups of about 20 *O*s at one time. The only instructions given were: "I want you to look at this group of drawings." The cards containing the figures were then exposed, one at a time, for 5 sec., until the entire series of 5 drawings had been presented. This was the only time the figures were seen. Twenty seconds after the series had been presented the *O*s were given blank paper and the instructions: "Reproduce the group of drawings that you just saw as accurately as you can." No instructions were given regarding the size of the reproductions with respect to the original. After 2 days the *O*s were told to reproduce the figures entirely from memory, with the same instructions as before, and again at 1, 6, 7, 14, and 19 day intervals. This meant that since the stimulus-figures were pre-

[8]The author wishes to express his appreciation to Dr. R. H. Wheeler for his suggestions concerning both the experimental technique and the writing of the results.

sented only once at the beginning, a series of reproductions was obtained that would indicate the undisturbed trend of the change with successive increments of time, with the last reproduction 49 days from the original presentation of the figure. Introspective reports were not taken.

Results

There were 3,559 reproductions of the 10 stimulus-figures. In all but a very few of these there was some change from the original that could be classed as greater than an error due to an inability in free-hand drawing. As would be expected in an experiment of this kind, the changes seemed on first inspection to be as diverse and complicated as the personalities of the individuals making the drawings. Inasmuch as the present problem was not concerned with a catagorical classification of the changes, which after all would be dependent on the material used and therefore only applicable to that experiment, no such attempt was made. Rather, the task was limited to the discovery of some general trend that would apply to all the memorial material and thus lead to a broader understanding of the problem of memory.

(*1*) *Changes toward symmetry.* It became evident from detailed examination of the data that all changes were in the direction of some balanced or symmetrical pattern, which was in part determined by the figure and partly by the perception of that figure. For this reason tabulation of the figures was made on the basis of the changes toward symmetry. There were a few reproductions early in the experiment that showed no change in any direction. The changes toward symmetry were recorded in tabular form for every one of the 10 figures. The individual changes for a particular figure fell into general classes. These classes for the individual figures seemed, again, at first, to be as diverse as the figures themselves, but on closer examination they all appeared as specific cases of 9 general groups of changes, which applied in most cases to all of the figures. These general classes are not proposed as categories, but represent trends of change that occur in the recall of the type of material illustrated in this experiment. They are intended not as explanations of the change, but as examples of the types of changes that appear. With this in mind we may proceed to a consideration of the changes toward symmetry.

(*a*) *Equalization.* A change in which corresponding parts of a figure, that were not equal in the original stimulus-figure, are made so in subsequent reproductions. In general this change took place in two ways in the present experi-

ment, first, in an equalization of lines in a figure, and secondly, in an equalization of angles. Practically all of the classes of changes mentioned in this section occurred in all of the figures, except where two changes were contradictory. In many cases several types of change appeared in the same reproduction of a given figure. Fig. 2, No. 1 gives examples of this type.

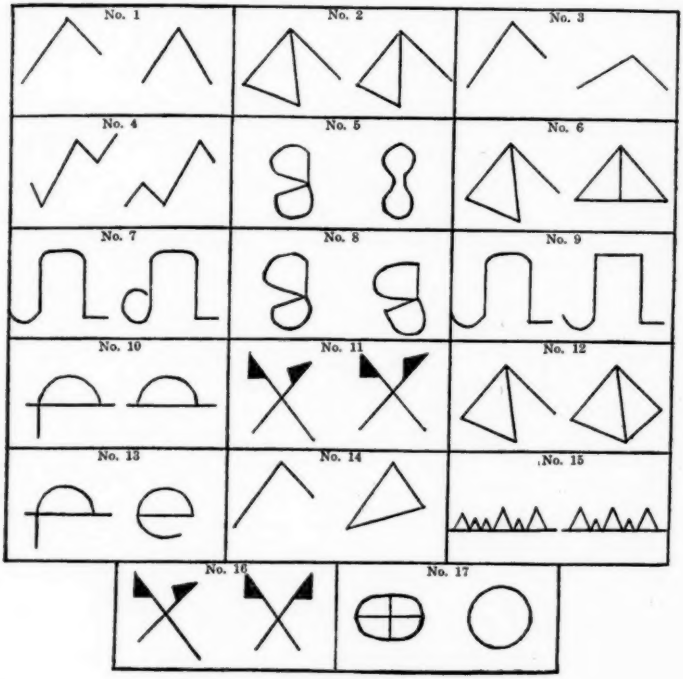

FIG. 2. TYPICAL CHANGES IN REPRODUCTIONS OF THE STIMULUS-FIGURES

The first drawing in every case represents the original figure, the second the reproduction of the figure illustrating various types of change.

(*b*) *Orientation.* Orientation may be a change that involves the whole figure or only a part of it. It may take any one of three forms. First, there may be a verticalizing of the figure or a part of the figure that was either horizontal or at an angle in the original stimulus-figure (Fig. 2, No. 2). Secondly, it may take the form of making the figure or a part of it horizontal when the original was not (Fig. 2, No. 3). Thirdly, the orientation change may be that of a reversal or inversion with reference to the stimulus-figure (Fig. 2, No. 4).

(*c*) *Standard figures.* This change is one that may take the form of a trend toward the symmetry of certain standard objects (glasses, dumb-bell, dollar sign). In this case the change is determined by the nature of the figure (Fig. 2, No. 5) and not by habit. The second type of change is that toward standard geometrical figures or parts of figures that are symmetrical (Fig. 2, No. 6). There are three special cases under this type of change that deserve special mention. The first of these is a change toward circles or parts of circles (Fig. 2, No. 7); the second is toward ovals or parts of ovals (Fig. 2, No. 8); and the third is a tendency toward squaring (Fig. 2, No. 9).

(*d*) *Simplification.* A change toward simplification of a given figure may assume two forms: first, the omission of parts or lines destructive to the symmetry of the figure—*i.e.* of parts that stand alone in the stimulus-figure, upsetting its balance (Fig. 2, No. 10); secondly, the decrease or shortening in successive reproductions of characteristics that upset the balance of the figure (Fig. 2, No. 11).

(*e*) *Complication.* A change toward complication is of necessity the opposite of simplification, and like simplification, may take place in two ways. First, there may be additions to the figure (Fig. 2, No. 12); and secondly, some characteristic of the figure may be increased, with subsequent reproductions, toward a more symmetrical pattern (Fig. 2, No. 13).

(*f*) *Completion.* A change toward completion of the original stimulus-figure may take place either in the addition of some new part of the figure or as an enlargement of some portion of the figure in making a closed and symmetrical form out of the original (Fig. 2, No. 14). The factor of completion depends of necessity on the nature of the figure involved.

(*g*) *Proportional relations.* Proportional relation of parts is a change which involves altering the position of some member, having no relation other than spatial to some corresponding part, so that the former is proportional to the latter (Fig. 2, No. 15).

(*h*) *Bilateral symmetry.* A change to bilateral symmetry occurs when the two sides of the stimulus-figure are nonsymmetrical in some detail and that detail is changed to correspond to the opposite side (Fig. 2, No. 16). It may be not only a change which involves the differing member, but may involve a change toward the differing member, or a change of both sides to a new form.

(*i*) *Whole symmetry.* Whole symmetry involves a change in the original stimulus-figure to a figure that, taken as a unit with all its members, is recognized as entirely symmetrical. It may occur in a change of a few details in the figure or it may be a change, involving the whole figure, toward an entirely new form (Fig. 2, No. 17).

(*2*) *Average change per drawing.* In tabulating the changes in the reproductions of the individual figures the necessary data were obtained to make possible a computation of the average change per drawing for successive reproductions of the 10 stimulus-figures. In doing this it was found necessary to compute the average time-intervals between the various reproductions of the different figures. This was necessary for the reason that some of the *O*s did not make all of the reproductions that were called for, which meant that the

time-intervals between the first and the second drawings would not be the same in one case as in another. For this reason the average time-intervals between reproductions are used in the data given. Fig. 3 presents graphically the percentage of change toward symmetry per drawing for each successive reproduction of the 10 figures. Both the A and B groups are included in this graph. The average cumulative time is given on the abscissae and the percentage of change on the ordinates.

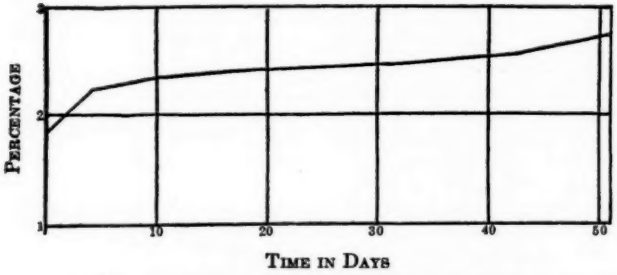

FIG. 3. PERCENTAGE OF CHANGE TOWARD SYMMETRY

It will be noted on examination of this curve that the greatest number of changes in the figures occurs on the first reproduction, that is, the greatest number of changes, in comparison to the increase with successive reproduction, appears in the reproduction that immediately follows the presentation of the stimulus-figures. The curve also shows that the increase of changes per drawing is proportionately greater in the first part of the experiment, and, although the changes per reproduction still increase as time goes on, the increase becomes less with each successive reproduction. There is, however, some indication that the increase in the number of changes becomes greater again toward the last of the experiment, after about 50 days (average cumulative time) from the presentation. These facts in all probability, mean with respect to the problem of memory, that at the first of the experiment there were a great many minor or smaller changes that came on the first or second reproduction, and thus account for the large increase at the first of the series. Whereas, as the experiment progressed, the changes that occurred were of greater significance, that is, involved more of the figure, and therefore of necessity could not be correspondingly as great in number. These later changes were also, as we shall point out later

on, more progressive in their development, which meant that the later changes were only gradual modifications of the large number at the outset. This would give meaning to the rapid increase in number of changes at the first of the experiment and the more gradual increase later on.

(3) *Figures with no change.* In tabulating the results of the various reproductions of the stimulus-figures not only the figures with changes toward symmetry were recorded, but in addition, the figures with no changes. Fig. 4 (dotted line) shows the graphic

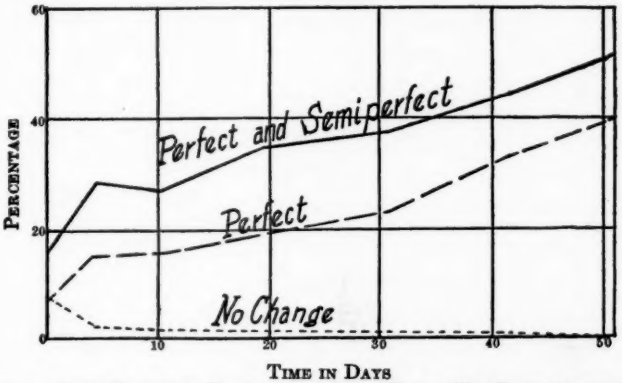

FIG. 4. PERCENTAGE OF REPRODUCTIONS IN WHICH 'NO CHANGE' WAS MADE, AND IN WHICH THE CHANGE WAS TO PERFECT SYMMETRY, AND TO PERFECT AND SEMIPERFECT COMBINED

representation of these results. The average cumulative time is represented on the abscissae, and the percentage of the total number of reproductions for a given day that show no changes is represented on the ordinates. It will be noted that even at the outset there is a very small number of figures with no change and that this initial percentage decreases very rapidly on the second reproduction. The percentage of the figures with no change on the succeeding reproductions is very small—1% or less. It is quite evident from this graph that there are relatively few figures with no change throughout the whole experiment and that these are confined for the most part to the first reproduction.

(4) *Symmetrical figures.* An independent grouping of the drawings was made in which the semiperfect drawings and the perfectly symmetrical drawings were recorded for each day of the series.

Fig. 4 (dashed line) shows the percentage of perfect figures out of the total number for any given time-interval. It will be noted that the percentage of perfect drawings increases gradually with the time-interval after the original presentation of the stimulus-figure. On the seventh reproduction, or 50 days (average cumulative time) from the original presentation, perfectly symmetrical figures occurred in approximately 40% of the drawings made at the time. Along with this curve is another (continuous line) that represents the addition of the perfect and the semiperfect figures. This curve follows that of the perfect figures very closely, except as would be expected, it is higher than the 'perfect' curve. On the seventh recall the percentage is 50% of the total number on that day. This

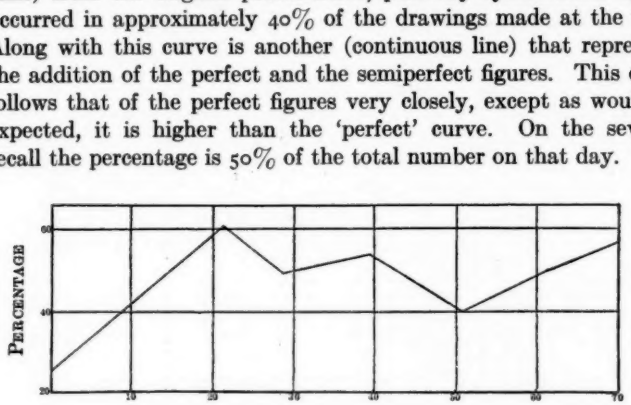

FIG. 5. PERCENTAGE OF FINAL REPRODUCTIONS APPROACHING PERFECT SYMMETRY

means, then, with respect to the present experiment, that each successive reproduction of a nonsymmetrical figure tends toward a symmetrical pattern, which involves the whole figure. That is, the tendency for the whole figure to change in an explicit fashion toward symmetry is more marked as time goes on.

(*5*) *Last symmetry.* In recording the perfect and the semi-perfect figures, a record was also kept of the figure's location in *O*'s series of reproductions. That is, if that particular perfect figure was the last in the series as far as that particular *O* was concerned, it was recorded. Here was a means by which to determine the percentage of the total number of last drawings that was symmetrical. Fig. 5 shows the results of this analysis of the data. The average time-intervals between these symmetrical drawings are given on the abscissae and the percentage of the total last drawings that were symmetrical is given on the ordinates. The curve ascends very rapidly from the first to the second reproduction. It will be noted, however, that

the greatest average time-interval occurs between the first and the second reproduction. The reason for this is that in this case the *O*s did not draw the figures again, after the first reproduction, until relatively near the end of the series. This would mean that the time-interval was longer. This high point on the second day is in fact the high point for the rest of the curve. The remainder of the curve descends, with time, until toward the end of the series when it again mounts to very nearly the point reached on the second reproduction. Interpreted, this means that in the recall of visually perceived forms the tendency toward symmetry is more marked when the reproductions are separated by a wide interval of time, than if the same time-interval is divided by a number of reproductions. The drawing of the material, then, must in itself serve as an added stimulation which cuts across the tendency toward symmetry, if those drawings are spaced relatively closely in time. The rise at the end indicates, however, that, although the foregoing statement holds, the symmetry tendency will appear, late in the series, even when the drawings are called for at fairly close intervals of time.

(*6*) *Progressive nature of change.* Probably the most significant fact that came to light in the present experiment was the progressive nature of the change toward a symmetrical pattern. If a particular change occurred at the outset it was progressively exaggerated or diminished, as the case might be, with each successive reproduction of the material (Fig. 6). For this reason many of the changes that occurred were trends in the direction of some symmetrical pattern, the culmination of which was never reached in the length of time used in this experiment.

We have already presented evidence which demonstrates that with a long interval of time between reproductions this progressive change is less gradual than when a number of reproductions are made over the same length of time. The curve giving the average change per drawing also corroborates this supposition. Whereas a large number of small changes are made at the outset, in the rest of the series the increase in the number of changes is more gradual. This means that the first changes are those which visibly involve minor parts of the figure, while those which come later involve more nearly the whole figure. Thus, by necessity, the later changes cannot be correspondingly as great in number as the minor changes, although the latter may still exist in the present figure.

The major changes, because of their comprehensive character, can only be a gradual modification of the changes that already exist. The changes that appeared at the outset are progressively modified in the direction of the symmetrical pattern, a pattern which involves the entire figure. As this progressive change continues in the direction of symmetry it involves certain minor changes toward symmetry.

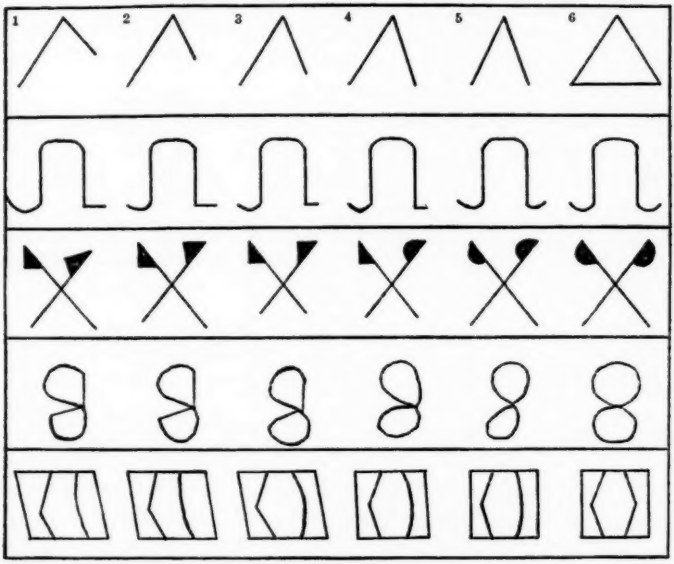

FIG. 6. EXAMPLES OF SUCCESSIVE REPRODUCTIONS
The numbers represent the position of the recall period with respect to the series.

That is, certain of the parts of the figure are involved in particular ways even in a general change that involves the whole figure. But as the general change is progressing gradually, the subordinate, part-changes emerge slowly and thus account for the smooth gradual progression of the curve depicting the average increase in changes per drawing with each successive reproduction of the figure. The progressive nature of the changes, the smooth character of the increase in changes, and symmetrical drawings, all represent evidence for some important suppositions about the memory process. They indicate that the energy pattern functioning in any memory process

is one that is dynamic in character and not static; a pattern that changes at a gradual rate and not by a sudden shift. The significance of such a supposition will be pointed out in a subsequent section of the paper.[9]

Discussion

Gibson's interpretation of the memory process. Gibson,[10] in his paper on the reproduction of visually perceived forms, following the work of Wulf,[11] finds certain points on which he would differ with Wulf in his interpretation of results, although there is nothing contradictory in the results themselves. The present experiment yields data in harmony with those of both preceding experiments, and in addition, seems to present evidence that would lead in the direction of the interpretation given by Wulf, not that given by Gibson.

The difference between Gibson and Wulf hinges about the concept of structurally conditioned change. Wulf claimed that certain changes which he observed were due to the *Struktur* or form of the figure. Inasmuch as this type of change seems to interfere with other changes attributed by Wulf to certain modes of apprehending the figures, Gibson argues, "this type of change is independent of the way in which the figure is perceived."[12] Although Gibson is no doubt correct in interpretating the form that Wulf speaks about as something which is not contained in the stimulus, but is phenomenological, he becomes ambiguous in assuming that the phenomenological and the perceived form are two different things. Gibson has overlooked the fact that phenomenological form, certain properties of which are determined by the way in which the figure is apprehended, is the *only* aspect of the object existing for the observer. The confusion is apparent in this quotation, "one is led to infer that the structurally conditioned change, then, is due entirely to this tendency towards a specific, lucid, complete, or 'good' *Gestalt*. The configurationist's *form* is apparently something which, in its own right and independent of the experience of the observer, possesses the capacity to change."[13]

[9]The reader will probably raise the question whether the same trend toward symmetry occurs in the recall of figures that are originally symmetrical. This question can be answered in the affirmative from various tests in the laboratory the results from which were so uniform and consistent that a systematic investigation seemed unnecessary. In successive recalls the patterns retained their symmetry but changed, in many cases, toward greater simplicity. In some instances, first reproductions made the figure more complex but the symmetry was always maintained. A change toward complexity can easily be explained in terms of the fact that the observer estimated the figure, originally, to be more complex than it was. Changes toward greater complexity generally occur when the figure is exposed for too short a time or when the observer examines the figure under distraction. In this general connection it is interesting to note that symbols, used commonly, are almost universally symmetrical, or nearly so. One does not change his memory of figures toward conventional symbols, because of habit, as Gibson implies, but the law of symmetry accounts for the habitual symbol.

[10]Gibson, *op. cit.*

[11]Wulf, *op. cit.*

[12]Gibson, *op. cit.*, 35.

[13]*Idem.*

Yet the *form* that Gibson is referring to is certainly not divorced from the experience of the observer, inasmuch as the experience provides the form. Perception and phenomenological form are two names for the same experience.

The issue is raised by Gibson; "Is the change in a reproduction of a perceived form caused by the influence of past perceptions on the perception and memory of that form, or is it a change caused by the nature of the form itself?"[14] No definite answer is given to the question. But again by implication the form is something over and apart from the perception. Gibson interprets *form*, as used by Wulf, as independent of the apprehension of form, and in addition, something which is divorced from the rest of the individual's experience. Such a dualism is an obvious fallacy. In fact the form *is* the apprehension of the object and is in no way something different in kind from the past perceptions which Gibson emphasizes. On the contrary, form is an evolutionary resultant in the continuity of the memory process.

As a substitute for the notion of form or structure, Gibson offers the following explanation of his results: "The changes observed in these experiments . . . are evidence for the existence of perceptual habits which have arisen in the individual during experience."[15] And further, "If, then, after an interval a reproduction of the figure is required, the assimilation becomes quite evident since the older traces, which fade more slowly than the new ones, are present to the exclusion of new ones."[16] It is questionable whether the changes mentioned by Gibson give evidence for the existence of old perceptual habits. We are not told exactly what a perceptual habit is, but Gibson later states that it is a process which takes place "by turning of a new perceptual process into earlier channels of perceptual activity."[17] The unanswered problem arises at once, What initiates and carries on this process of turning? Obviously Gibson must assume something more before he can approach an explanation, a something which will necessarily be the *sine qua non* of his theory. This something is adequately explained by the configurational assumption of a dynamic field functioning under the laws of fluid energy.

Gibson is in further difficulty. How is one to account for the fact that the number of changes *increase* as time goes on, and are a *progressive modification* of the earlier changes? From his statement that the changes are to be explained in terms of old perceptual habits, the only logical suppositions to make are that, first, a*ll* of the changes in a particular figure, showing any change at all, should appear on the first reproduction, and secondly, that all of the figures should change to the maximum on the first reproduction. Since none of the facts from Wulf, the present experiment, or even from Gibson's own experiment support this position, the theory must obviously be inadequate.

Gibson falls back, finally, upon a trace theory of memory. He says that, "the older traces, which fade more slowly than the new ones, are present to the exclusion of new ones."[18] It is impossible, from Gibson's discussion, to determine just how he envisages these traces in the organism. The absurdity of a purely structural trace, of course, has been evident for nearly two centuries. This Gibson seems to realize, for he promises us a functional explanation. But he then presents the theory that new perceptual activity takes place in terms of old perceptual habits and that these new processes are turned into earlier channels of

[14] *Idem.* [15] *Ibid.*, 39. [16] *Ibid.*, 38. [17] *Idem.* [18] *Idem.*

perceptual activity. An explanation which interprets events in terms of the processes that have preceded them, is, however, a long way from a functional viewpoint; in fact, it is only a restatement of the static trace theory. If everything new is to be explained in terms of the old, wherein is it dynamic? The most significant facts that we know about the memory process, those of directional change and newness, must of necessity be disregarded by Gibson's position.

"The notion of an old memory image fusing with a new perception, and the notion of an old configuration asserting itself over a new configuration differ essentially only in the terms used," is an assertion of Gibson.[19] His own logic, has, however, led him astray. He fails to interpret correctly the position that Wulf is taking. The *Gestalt* position would make it logically impossible for an old configuration to exist with reference to the organism, inasmuch as the only configuration that exists for the organism is the one *in the present.* That is, although the present configuration is a continuation from the past of the organism, it in no sense is caused by the past,—or is it old. This does not mean that there is an isolated configuration every second, but means that the configuration is both a spatial and temporal *unit.*

A new theory of memory. Certain facts have come to light in this experiment that suggest a general theory of memory. Such a theory, to be adequate, however, must fulfill certain requirements before it can pretend to cope with the problems in this field. (*1*) The explanation must ultimately be based on the concept of mental continuity. In fact memory and mental continuity are different aspects of the same problem. (*2*) It must be a dynamic concept in order to account for the progressive nature of the changes that occur in recall. (*3*) It must account for the direction of the changes. (*4*) It must account for the increased changes occurring with successive increments of time.

(*1*) The first problem to consider is that of mental continuity, more specifically that continuity implied between original observation and recall. How can mental continuity be envisaged in organismic terms? Observation and subsequent recall are members of a *temporal configuration.* By a temporal configuration is meant a given whole in mental life, any part or experience of which has significance only in its relationship to the total continuum within that whole. No one phase of the continuum will explain any other. The observation will not account for the recall. A past experience will not account for a present one. The two *depend* on each other, not in terms of cause and effect relations, but *by definition.* Both are necessary before there is anything to explain. The one does not exist without the other for the same reason that the first half of an inch, by definition, cannot exist without the other half. Both, in either case, belong to the same descriptive unit.

The problem then becomes; How may we account for this unit physiologically? We conceive of the physiological growth of the organism as a gradual,

[19] *Idem.*

evolutionary process, going on under a growth potential and directed by an alignment of stresses which Child calls a physiological gradient.[20] In this growth process the particular specialized structures develop not by the addition of new entities, but as emergence phenomena differentiating from a relatively unspecialized field of energy. Likewise, it is supposed that the mental development of the organism is due to the same maturation potential. Growth is a variety of motion; it is the growth potential realized in kinetic energy. Some of this energy is realized in the growth of organic structure, some in overt bodily movement, and some in mental development. Because it is stimulation that induces the differentiation of the potentials, observation and recall are merely phases of this development separated by removal of certain modes of stimulation.

Maturation, with respect to a given object to be recalled, is a continuation of that particular process of differentiation, the emergence phenomena continuing with each recall. Mental continuity is then accounted for by the fact that although the organism is different every second of its existence, there is no break between what it is one second and the next because the realignment of stresses is an unbroken continuum, even between observation and recall. So far we are in accord with the logic of Köhler's theory of the non-process change.[21]

The organism, however, is at each instant a unitary intact whole in space and in time, the parts and temporal phases of which do not combine to form the whole. The experiences that the organism has had are represented in this unitary whole only in terms of the general differentiation of the neural field, that is, in a level or degree of maturation. General structurization of the entire field is the only way in which maturation is represented. Specific structurization exists only with reference to the stimuli of the moment and passes with specific modes of stimulation. In other words, the high and low potentials, corresponding to specific experiences existing at any instant, are not represented by permanent structural changes in the nervous system. Rather, one has an experience as the potentials of the nervous system are realigned in terms of existing stimuli. There is a resolution of this potential and the experience terminates.

The most important point to bear in mind is the maintenance of dynamic balance, or movement in the direction of balance, all the while structurization is occurring. The preservation of dynamic relations *during* processes of change, when a process of change occurs in the course of reëstablishing a balance, is hard to understand and even more difficult to picture. This preservation of dynamic relations during change is the physiological basis of continuity in the maturation process, and is the only representation of the organism's 'past experiences.' But note, the dynamic situation is constantly changing, and is general in character.

What, then, accounts for the reference to the past, the feeling of familiarity, so often regarded as the phenomenological criterion of recall? There are no traces. There is nothing in the preservation of dynamic relations in a changing field that guarantees familiarity. Each feeling of familiarity represents the temporary emergence of a specific quality from the existing conscious field, integrated with the field as it emerges. The emergence is induced by the stimulus-

[20]C. M. Child, *Physiological Foundations of Behavior*, 1924, 2.

[21]Wolfgang Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175. For a critical discussion see R. H. Wheeler, *The Science of Psychology*, 1929, 276 f.

pattern of the moment. Feeling of familiarity, or reference to the past, is no different, functionally, from reference to the future, or feeling of strangeness. Both are new responses organized with reference to the goals of the moment, and are both on the order of discoveries, involving insight into total situations.

The problem of reference to the past and reference to the future is no more solved by traces than is the problem of a difference between seeing red and seeing green. If we must have a trace to account for reference to the past, by the same token we need a trace to inform us when we see red, another to tell us an object is green, and so on. We have then shifted all mental processes, as emergent phenomena, back to the nervous system and are no farther along than when we started. We have made the trace accomplish what mental processes are supposed to accomplish, inform us of meanings.

Finally, new experiences are logically prohibited by a trace theory. If experience could change, the extent of the change must be known to the observer, and a comparison made with the original experience. This is because the same trace that has lasted over has been changing. Under these conditions, also, nothing different from the original experience would ever be reproduced. A changing or fading trace is a logical contradiction in terms.

To illustrate from the present experiment the preservation of dynamic relations in a changing field, consider the observation and recall of any particular figure. The retina is subject to excitation by a certain arrangement of stimuli, disequilibrating preëxisting stress-patterns in the brain. The process of reestablishing a balance corresponds to the perception of the figure. As long as the stimulus continues the disequilibration continues and the perception, likewise, in the course of reëquilibration. The stimulus is removed and the perception ceases. But the figure itself was by no means the total stimulus-pattern in terms of which the perception was formed. The perception of the figure emerged from a ground which was sustained by the remainder of the stimulus-pattern. The ground is represented in the stress-patterns previously mentioned, the figure and the ground being represented by a gradient with the figure corresponding to the region of highest potential in the gradient. Stimuli sustaining the ground were the classroom situation, the presence of the instructor and students, words used by the observer to describe the pattern as he was observing it, and instructions given by the experimenter.

Assuming that the observer does not think of the figure after it is removed, what goes on between this period of observing and a subsequent recall? The observer goes about responding to other stimulus-patterns, using the nerve structures for other purposes that were formerly employed in observing and will be employed in recall. The neural field is constantly being subjected to disequilibration and is constantly releasing its potentials in the course of reëstablishing the balance. With different stimulus-patterns, parts of the field are required to balance themselves in different ways, so that those parts of the field that shall have the highest potentials vary at different times. These must all find their balance, but, in each instance, the balance will be a different one. This continuous change toward balance, while at the same time the balance required is constantly changing, is the essence of maturation. Thus, by the time a recall situation is introduced, the stimulus-pattern setting up the recall will not have the same field to disequilibrate as did the original stimulus-pattern. But the

original stimulus-pattern and the subsequent partially repeated stimulus-pattern are conditions in common both to the observation and to the recall. Thus the reason why a person is able to recall an experience is, to this extent, the same as the reason why he was able to make the original observation. The one does not depend on the other in a causal sense but both depend upon similar conditions.

Now we have the setting for the recall. The process of recall is induced (the observer coöperating) by portions of the stimulus-pattern similar to the original; but the figure itself is absent.[22] The field-arrangement of stresses, now resting upon a different base (different level of maturation), will be somewhat like the original. Incompleteness of the stimulus-pattern and an alignment of stresses upon a different base account for the incompleteness of the reproduction as compared with the original. Since, functionally, the organism is very nearly the same, that is, the maturation base maintains a certain constancy of major characteristics in the form of dynamic relationships of various parts of the nervous system to each other, the partially repeated stimulus-pattern has something of the same dynamic relation to the whole as did the original. This actual correspondence is the situation that makes possible an organized, insightful response of the observer to the repeated situation. The phenomenological characteristic of this response is his feeling of familiarity or reference of his experience to the past. Constant major characteristics of the maturation base, a continuous adjustment of a particular organism to its own environment, account for the fact that the memories for one person are different from the memories of another. In reality there has been no more of a trace than if the observer was confronting a novel situation and insightfully perceived it as new. For in the case of a novel situation a certain dynamic relation between the organism-as-a-whole and the stimulus-pattern must also be the starting point of the new judgment. Judging an experience as old is, in this same sense, a new judgment, akin to an invention.

(*2*) The progressive nature of changes in recall, and the direction of these changes, are to be explained in terms of least action.[23] The changes in successive recall were all toward symmetry. Why? Here, as Wulf would say, the answer lies in the laws of *Prägnanz*, which, we believe, should be interpreted in terms of least action. The most stable energy patterns are those which are structured symmetrically, the most stable form of all being a perfect sphere. It is also the pattern that requires the least energy to maintain. The predominance of symmetry, especially in simple forms in nature bears out this point.

Other things being equal, therefore, an observer will not only perceive symmetrical and balanced stimulus-forms more easily than others, but he will reproduce them more easily. In terms of least action the pattern of disequilibration set up by an asymmetrical stimulus will ordinarily assume more symmetry than corresponds to the original arrangement of stimuli. Thus the observer will be inclined to see the figure more symmetrical than it is.

(*3*) But we have not answered the question, Why are the errors of recall *progressively* in the direction of symmetry and simplicity? The difference in energy values between the high and low potentials of the field, corresponding to

[22]For a theory based on the likeness between the original stimulus-pattern and a later one see Wheeler, *op. cit.*, 271 f.

[23]For a general application of this law to all behavior see Wheeler, *op. cit.*, 79 ff.

the figure and ground of the recall process, becomes greater with each successive recall. There are two outstanding reasons for this situation. First, the farther the observer is removed, in time, from the original stimulus-pattern, the fewer are the stimuli in the recall situation that duplicate those of the observational situation. Secondly, the longer the elapsed time between observation and recall the more the maturation base has changed.

Imagine, now, an observer 'set' to recall under these conditions. The 'set' is the stress induced by instructions, or some suggesting factor, and exhibited phenomenologically in the effort or will to recall. But in terms of what has just been said it is more difficult than ever to resolve this stress in terms of the original stimulus-situation. Because the difference in energy values is now so great between the high and the low potentials of the system which must function in the recall, much more energy is required in shifting from the one to the other. The resolution will, therefore, take its course over a shorter and simpler route than heretofore. In other words, the reproduced figures will approach greater symmetry and simplicity.

(4) When the difference between the high and the low potentials becomes sufficiently great the recall situation, meager in stimulation, releases a limited amount of energy. All of this energy is expended in the recall, but since so much of it is required in the resolving process, little remains for the finished reproduction. This situation exaggerates, in terms of least action, the symmetry and simplicity of the figure.

This explanation accounts for the difference in symmetry and simplicity found in recalls close in time to the original observation, and recalls farther removed in time without intermediate reproductions. We may suppose that the energy value between the high and low potential during immediate recall is low and that the energy value between the high and low potentials in delayed recall is high. But this does not mean that the amount of energy available for expenditure during recall is different in both cases, for the energy of both is the energy of the total field. In fact the energy that maintains the figure in immediate recall is much greater because energy not expended in resolving the stress is used in maintaining a more complicated figure. In the delayed recall, however, more of the energy of the entire field is preëmpted in the resolution of the differential between the high and low stress and there is less energy to maintain the figure. The figure will, therefore, be simpler and more symmetrical. In each case the recall, with respect to the field as a whole, has been an all or none process.

The structurization of the field as a whole, in the course of maturation, may complicate matters considerably, leading in some cases to the availability of more energy and therefore to the reproduction of a figure of increased complexity either over the original or over a previous recall. Or, it may lead to numerous qualitative changes in the direction of other symmetries than the one toward which the observer was at first progressing.

Summary

(1) The voluntary reproduction, from memory, of visually perceived forms undergoes a change toward symmetry and simplicity.

(2) The change toward symmetry varies, under the conditions of the present experiment, in proportion to the interval of time

between original observation and first reproduction, or between one reproduction and another.

(3) The change toward symmetry is progressive with successive reproductions.

(4) Based on the results of this experiment a general theory of memory is proposed that emphasizes the following principles: (a) The temporal unity of the memory process precludes the possibility of explaining recall in terms of observation. Rather, recall and observation are, like two halves of a whole, dependent by definition. Factors explaining memory are external to a temporal configuration, structured with respect to time, involving both the recall and the observation. (b) Memory is based on maturation and partially duplicated stimulus-patterns. The feature of maturation important for the phenomenon of memory is the preservation of dynamic relations in a changing energy field, that is, a continuous change toward balance while at the same time the balance required is constantly changing. The details of this process are explained in terms of physiological gradients, high and low potentials, and the laws of the resolution of differentials. These are, first, the law of least action, and its special case: the demand for stability of pattern in symmetry and simplicity of form; and secondly, the law of maximum work which is the all-or-none principle applied to energy fields.

A COMPARISON OF INDIVIDUALS AND SMALL GROUPS IN THE RATIONAL SOLUTION OF COMPLEX PROBLEMS

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The work done before 1920 in the field of group activity as compared with individual activity has been summarized adequately by Allport.[1] Since then other studies have been made principally by G. B. Watson,[2] Bechterew and Lange,[3] South,[4] and Belaev.[5] Many of these studies deal primarily with groups wherein individuals are interested in the same stimulus but there is little or no direct social intercourse and stimulation. The work of these individuals is contrasted with that done by isolated individuals.

The present study aimed to present individuals with an actual problematic situation which would call for real thinking to arrive at a proper solution. The problems selected (given in detail below) involve a number of steps all of which must be correct in order that the right answer may be found. The problems were to be given to single individuals and to small groups of coöperating individuals, in order that the abilities of these two might be compared. The problems were such as to make it practically impossible for one individual in the group to get a correct answer instantaneously by a sudden insight into the situation. Rather, they allow all to participate in arriving at a solution; they call for interchange of ideas and for acceptance or criticism and rejection of any idea put forth. At least some of the previous studies have dealt more with the performance of

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[1] F. H. Allport, *Social Psychology*, 1924, Chap. II.

[2] G. B. Watson, Do groups think more efficiently than individuals? *J. Abn. Soc. Psychol.*, 23, 1928, 328-336.

[3] W. Bechterew und A. Lange, Die Ergebnisse des Experiments auf dem Gebiete der kollektiven Reflexologie, *Zeit. f. angew. Psychol.*, 24, 1924, 305-344.

[4] E. B. South, Some psychological aspects of committee work, *J. App. Psychol.*, 11, 1927, 348-368, 437-464.

[5] B. V. Belaev, abstract received from the author, entitled "The Problem of the Collective and its Internal Structure." Other references are: W. A. Barton, Jr., Group activity versus individual activity in developing ability to solve problems in first year algebra, *J. Educ. Administration and Supervision*, 12, 1926, 512-518; H. E. Burtt, Sex differences in the effect of discussion, *J. Exper. Psychol.*, 3, 1920, 390-395; M. P. Follett, *Creative Experience*, 1924; K. Gordon, Group judgments in the field of lifted weights, *J. Exper. Psychol.*, 7, 1924, 398-400; A study of esthetic judgments, *ibid.*, 6, 1923, 36-43; E. C. Lindeman, *Social Discovery*, 1924; A. D. Sheffield, *Creative Discussion*, 1927; T. W. Thie, Testing the efficiency of the group method, *English Journal*, 14, 1925, 134-137.

certain rather elementary tasks such as word-building, vowel cancellation, multiplication, turning fishing reels, etc., which have allowed but little constructive coöperation wherein ideas may be accepted or rejected either in whole or in part.

The problems used here are still quite far from the usual type of problematic situation met in real life. They are problems admitting of only one answer; there is only one type of best solution. They arouse none, or very little, of the emotional bias with which persons characteristically approach life situations. It can be seen at once, then, that not all of the results obtained are directly applicable to any or all group situations.

Method and Procedure

Subjects. The *Ss* in this experiment were the students in the class in Social Psychology at Columbia University. This group is a rather highly selected one, in view of the fact that almost all are graduate students, a large percentage of them working for advanced degrees. The experiment was divided into halves; 3 problems were used in the first half and 3 in the second half given 2 weeks later. In the first half there were 2 groups of 4 women each and 3 groups of 4 men each. There were 9 men and 12 women working on the problems individually in the same room. In the second half of the experiment there were 2 groups of 4 men each and 3 groups of 4 women each; and 10 men and 7 women worked as separate individuals in the same room. Thus data were received from an equal number of groups of men and women, *i.e.* 5 groups of each. It will be seen from the above that a group was never composed of the two sexes, but in all cases of either 4 women or 4 men. It was believed that this arrangement would, in general, make for better coöperation and more smoothly running groups. The grouping was not made for any purpose of sex comparisons; no such comparisons are made in this study.

Problems. (*a*) *First half of experiment.* The directions used in the first half of the experiment were as follows:

(1) Materials for this problem are in the envelope marked 'Problems I and II.' Use disks H1, H2, H3, W1, W2, W3. (For the present disregard the symbols on the reverse side.) Side 1 of the card. On the A-side of a river are three wives (W1, W2, W3) and their husbands (H1, H2, H3). All the men but none of the women can row. Get them across to the B-side of the river by means of a boat carrying only three at one time. No man will allow his wife to be in the presence of another man unless he is also there.

(2) Materials for this problem are in the envelope marked 'Problems I and II.' Use disks marked M1, M2, M3, C1, C2, RC. (Reverse side of the disks just used.) Side 1 of the card. Three Missionaries (M1, M2, M3) and three Cannibals (C1, C2, RC) are on the A-side of a river. Get them across to the B-side by means of a boat which holds only two at one time. All the Missionaries and one Cannibal (RC) can row. Never under any circumstances or at any time may the Missionaries be outnumbered by the Cannibals. (Except of course, when there are no Missionaries present.)

(3) Materials for this problem are in the envelope marked 'Problem III.' Side 2 of the card. In Circle A arrange the disks in order of size, that is with the largest on the bottom, etc., ending with the smallest on top. Using Circle B as a transfer station, transfer the disks to Circle C so that they will be in the same

order in Circle C that they are now in in Circle A. Never place a larger disk on a smaller one and move only one disk at a time. (Number the disks for reference if you wish.)

The card referred to in these problems contained on the one side a diagram of a river, for convenience in solving the first two problems, and on the other side a diagram of the three circles necessary to a solution of the third problem.

(*b*) *Second half of experiment.* The directions used in the second half of the experiment were as follows:

(1) Materials for the problem are in the envelope marked 'Problem I.' Put these words, taken from the envelope, together so that they form the last sentence (only one sentence) of the unfinished prose selection.

In New Orleans there is a tree which nobody looks at without curiosity and without wondering how it came there. It reminds one of the warm climes of Africa and Asia. Indeed, with its sharp and thin foliage, sighing mournfully under the blast of one of our November northern winds, it looks as sorrowful as an exile.

(2) Materials for this problem are in the envelope marked 'Problem II.' These words when put in the proper order form the last three and one-half lines of the unfinished sonnet below. Arrange them as nearly as possible in the proper order.

A boy named Simon sojourned in a dale;
Some said that he was simple, but I'm sure
That he was nothing less than simon pure;
They thought him so because forsooth, a whale
He tried to catch in Mother's water-pail.
Ah! little boy, timid, composed, demure—
He had imagination. Yet endure
Defeat he could, for he of course did fail.
But there are Simons of a larger growth,
Who, too, in shallow waters fish for whales,
And when they fail

(3) A consolidated school is to be built in the rural district shown in the diagram. The capital letters (A, B, C, etc.) indicate points (not towns) where pupils are to be picked up by two school buses. The mileage between each point is indicated on the diagram. The capacity of each bus is 35 pupils and the driver. Find the most desirable location for this school and give the route each bus must take. The buses may start at ANY point and need not necessarily start from the school each morning. Following are the number of pupils to be picked up at each point:

Point	No. Pupils	Point	No. Pupils	Point	No. Pupils
A	6	D	4	G	3
B	13	E	2	H	10
C	17	F	5	I	3

The diagram for problem 3 is given below. It was furnished to each individual who was working alone and was drawn to the scale of 1 in. to the mile. Only one diagram was provided in each group; it was drawn to the scale of 2 in. to the mile.

In addition to the specific instructions given above, the following general instructions appeared at the top of each set of problems given the separate individuals working in the same room.

Below are three problems. Work them as quickly and accurately as possible. There is a correct solution to each problem. Record your answer or state briefly how you solved the problem. When you have finished one problem and are ready

to go on with the second, record the time to the nearest half-minute by means of the record being kept on the front board. Then proceed with the second and then the third, and record the time when each is finished. Work the problems in the order listed.

At the top of the set of problems given to each group of 4 individuals were the following instructions:

A chairman has been appointed to manipulate the necessary materials. Work together as a coöperative group to solve the three problems given below. Work them as quickly and accurately as possible. There is a correct solution to each problem. Record your solution or state briefly how the problem was solved. Each individual, including the chairman, should make his contributions to the group solution spontaneously as they occur to him. Indicate to the note-taker when you have finished one problem and are ready to proceed to the next; the note-taker does not participate in any way as a member of the group in solving the problem.

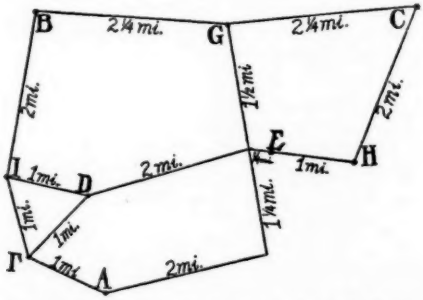

FIG. 1. DIAGRAM USED IN PROBLEM 3, SECOND HALF OF THE EXPERIMENT

Every individual in the group was provided with a separate set of the problems, but only one set of the necessary materials was given to each group; this made it more essential that all coöperate to solve the problems. The first individual in each group of 4 was appointed chairman to manipulate the materials in order to obviate the confusion and delay which might result if each attempted to carry out his own ideas. The chairman was appointed rather than being elected by the group to prevent unnecessary delay in starting to work, and because in many cases the individuals had never before met except in the more formal classroom atmosphere. It was thought that any formal or parliamentary organization of the group might tend to restrict spontaneity and coöperation and thus perhaps limit the possibilities of getting the best results.

The note-taker, referred to in the group instructions above, was for the purpose of securing more qualitative and quantitative facts concerning the group activity than the time of solving and answers to the problems would provide. In the first half of the experiment the note-taker was given the following instructions.

You are to act as note-taker and time recorder for a group of persons solving three problems. You are to take absolutely no part in the group activity. Do not indicate by any means your approval or disapproval of their activities.

Record the final solution of the group on each problem. Record the time required to solve each problem. Record as many of the detailed suggestions made by any member of the group as it is possible for you to get.

In the second half it was thought that the following plan might afford more quantitative data concerning the activity of the groups.

Tally separately every suggestion made by any member of the group so that a record can be made up of the complete number of suggestions made in any one group. Get as much as is possible of each suggestion made. Record this in your notes. If the suggestion is rejected check the tally. Note whether it is rejected by the person who proposed it or by another member of the group. If the suggestion is accepted underscore the tally.

There were 10 sets of notes taken and in only 3 cases did the note-taker have any knowledge either of the problem or of its correct solution. Thus the knowledge of whether a rejected suggestion was in reality an erroneous or a correct suggestion could have but little effect on the notes taken, and can be determined by *E* only from these same notes.

Equalization of the groups. In order to equate the groups, that is, to be reasonably sure that no one group was made up of four superior individuals and that those who worked in groups were on the whole neither inferior to nor superior to those working as separate individuals, shifts were made so that the composition of the groups differed in the two halves of the experiment. Individuals making up the first group were chosen from the class roll by placing every other man and woman in a group. Certain deviations had to be made from this general procedure in the case of absence from class, since the aim was to keep the total number of men working in groups and as separate individuals approximately equal for the total experiment; the same being true for the women. Every group was composed of 4 individuals because it was thought that a group of this size would coöperate to better advantage than a very much larger or smaller one. Also the limited number of available *S*s made it impossible to increase the number much and still have enough single groups to make the comparisons meaningful. Limited room accommodations also limited the number of groups.

Table I below shows the accomplishments of every *S* as he worked alone in the first half of the experiment. Table II shows the accomplishment of the groups in the second half of the experiment. Group A′ in the second half of the experiment was composed of individuals *Y*, *G*, *I*, and *R* of the first half of the experiment; Group B′ of *K*, *U*, *W*, and an individual not before present; Group C′ of *M*, *J*, *A*, *F*; Group D′ of *Q*, *L*, *R*, *C*; and Group E′ of *H*, *T*, *X*, *B*. It will be seen that each group contained only one person who had solved a problem correctly in the first half of the experiment. Since the groups in the first half were chosen at random it was necessary to check the individuals in the second half to see that no group in the first half had been superior. From Table II can be seen the accomplishments of the individual *S*s in the second half. Group A was made up of individuals *A′*, *J′*, *N′*, and an *S* not used in the second half; Group B of *H′*, *F′*, *M′*, and *P′*; Group C of *B′*, *C′*, *X′*, and an *S* not used in the second half; Group D of *E′*, *K′*, *L′*, and *Q′*; and Group E of *D′*, *G′*, *I′*, and an *S* not used in the second half. Thus it seems probable that no group was composed of 4 superior individuals. In neither half of the experiment

did a group contain more than one individual who had solved a problem correctly in the other half of the experiment, and no single individual ever correctly solved more than one problem.

TABLE I

SHOWING FOR EVERY INDIVIDUAL *S* AND FOR EVERY GROUP OF *S*s THE TIME IN MINUTES REQUIRED FOR A SOLUTION AND THE NATURE OF THE SOLUTIONS OF THE PROBLEMS OF THE FIRST HALF OF THE EXPERIMENT

		Problem 1		Problem 2		Problem 3	
		Time	Solution	Time	Solution	Time	Solution
Individuals	*D*	6.5	Incorrect	29.0	Incorrect	6.0	Incorrect
	F	4.5	"	17.5	"	13.5	"
	G	3.5	"	6.0	"	20.5	"
	H	1.0	"	3.5	"	10.0	"
	K	2.0	"	4.5	"	18.0	"
	L	1.5	"	2.5	"	9.5	"
	N	1.0	"	2.5	"	15.0	"
	P	7.0	"	5.5	"	15.5	"
	R	1.5	"	3.0	"	19.0	"
	A	6.0	"	15.0	"	14.5	Unsolved
	C	4.5	"	5.5	"	14.0	"
	W	6.0	"	18.0	"	14.0	"
	J	4.5	"	6.5	"	5.5	"
	X	4.0	"	12.5	"	13.0	"
	Y	4.5	"	10.0	"	15.5	"
	T	10.5	"	20.0	"	15.0	"
	B	3.0	"	8.5	"	60.0	Correct
	I	4.5	"	8.5	"	21.0	"
	M	5.0	Correct	8.0	"	5.0	Incorrect
	Q	9.5	"	9.5	"	5.5	"
	U	4.0	"	12.5	"	16.0	Unsolved
	Av.	4.5		9.9		15.5	
	S.D.	2.5		6.7		11.0	
Groups	A	5.0	Incorrect	7.5	Incorrect	4.5	Incorrect
	B	4.5	Correct	34.0	Correct	17.0	Correct
	C	5.0	"	12.0	"	37.0	Unsolved
	D	16.0	"	27.0	"	29.0	Correct
	E	2.0	Incorrect	4.0	Incorrect	4.0	Incorrect
	Av.	6.5		16.9		18.3	
	S.D.	4.97		11.6		13.1	

RESULTS

Table I summarizes the time (in minutes) required for and the nature of every solution presented by a group or by the individual *S*s in the first half of the experiment. It will be seen that only 5

correct solutions out of a possible 63 were returned by the different *S*s. There were 8 correct solutions out of a possible 15 for the groups. That is, 7.9% of the solutions turned in by individual *S*s were correct, while 53% of the solutions turned in by groups were correct. It was found that the false solutions to the problems could be divided into several different types. In Problem 1 two types of false solution, handed in by 13 different individuals, made an error in the first move, 4 made an error in the third move and 1 made an error in the fifth. It took 7 single moves to solve the problem correctly. No group erred on the first move; one erred on the third and one on the fourth. Group activity would seem to insure not only a larger proportion of correct solutions, but also that even in erroneous solutions the error is not made so early in the solution.

The solution to Problem 2 shows the same. It takes 13 single moves to arrive at a correct solution. Among the individual *S*s, 3 erred on the first move, 4 on the second, 6 on the third, 2 on the fifth, 2 on the sixth, 1 on the seventh, and 2 on the eighth. The last 3 who erred on the seventh and eighth, did not consider their solutions completed as all the cannibals and missionaries had not been transferred to the B-side of the river. It is interesting to note that no group got as near as this to a correct solution of this problem and then failed to reach it. Three groups solved the problem correctly; both of the others erred on the fifth move.

Problem 3 cannot be classified so easily on the basis of the first false move, since in all cases, except in one group, once the method of transfer was hit upon success was assured. In the case of 7 individual *S*s no solution at all was recorded; it may be assumed that they were unable to reach one. Six individual *S*s skipped circle B, the transfer station. Two spread the disks out in B until 4 had been transferred; then the disks were piled up and the fifth one either transferred through B to C or skipped over B to C. After this the others were again spread out and then transferred to C. Four individual *S*s slipped the disks from underneath. One group skipped the transfer station; one spread the disks out as described above. No group slipped them from underneath; 2 groups contemplated this but abandoned it as being too simple; one group abandoned spreading them out as also being too simple. In Group C, Problem 3 has been recorded as unsolved, but this does not fairly represent the group. They had successfully transferred 4 disks into circle C and the fifth into circle B, but in manipulating to get the fifth disk into C they lost sight of their real aim and became confused. They quit work then,

although saying that they 'probably could figure it out if they stayed with it long enough.' The instructions for this problem appear not to have been sufficiently clear in the prohibition of certain procedures. It was felt that too many instructions here might make the correct solution too obvious, although the bizarre plan of spreading the disks out in the circles was never anticipated. The problem had been previously tried out both on groups and separate individuals, and neither the plan of spreading out the disks nor that of slipping them from underneath had occurred. The instructions had, therefore, been assumed to be adequate. It is to be noted that several *S*s indicated that they were aware of not having followed the directions exactly. For example, one individual who slipped them from underneath commented, "This seems too simple but I can't see how it can be solved without moving a covered disk." Also, after the experiment, one individual *S* was overheard by *E* saying that "since the instructions did not say that the disks should not be slipped from underneath" she solved the problem that way because she "could not see how else to do it."

Table II gives the time and number of errors made by every individual *S*, and by every group of *S*s on the three problems given in the second half of the experiment. The seventh column, which is headed "location," gives the number of miles which the proposed location is from the best location. The eighth column gives the number of excess miles which the two buses travel, the shortest possible number of miles being 12.

It will be noted that here, as in Problem 1, a greater percentage of the groups obtained a correct solution than individual *S*s. Of the solutions reported by the different *S*s 5.7% were correct; and by the groups, 27% were correct.

The incorrect solutions in this half of the experiment also are interesting when analyzed. Among the groups there were 4 correct solutions on the first problem, the single error being the transposition of the words "time immemorial" to read "immemorial time." Only 3 *S*s completed the sentence to read as it had in the original. By far the greatest number of errors lay in the placing of the single word "there," which could be put in 7 different places without disturbing the smoothness of the sentence. This error was made (often together with others) in 10 cases by individual *S*s but by no group. Five *S*s submitted solutions wherein the structure of the sentence was poor.

TABLE II

SHOWING FOR EVERY INDIVIDUAL *S* AND FOR EVERY GROUP OF *S*s THE TIME IN MINUTES REQUIRED FOR A SOLUTION AND THE NUMBER AND NATURE OF THE ERRORS MADE IN THE PROBLEMS OF THE SECOND HALF OF THE EXPERIMENT

		Problem 1		Problem 2		Problem 3		
		Time	Errors	Time	Solution	Time	Location	Excess
Individuals	*A'*	6.0	2	27.0	Unsolved	5.0	1.25	0.0
	B'	8.0	1	37.0	"	21.0	.125	.5
	C'	12.0	10	150.0	Incorrect	35.0	.25	.5
	D'	7.0	8	25.0	Unsolved	25.0	1.0	.5
	F'	8.0	1	16.0	Incorrect	16.0	1.25	.5
	G'	2.0	1	6.0	Unsolved	13.0	3.0	.5
	H'	14.0	1	20.0	"	15.0	.25	.5
	J'	6.0	1	85.0	Incorrect	30.0	6.0	1.75
	K'	9.0	2	35.0	Unsolved	12.0	4.0	1.75
	L'	7.5	1	20.0	"	24.0	6.0	1.75
	M'	11.0	7	42.0	Incorrect	18.0	1.25	1.25
	X'	12.0	3	39.0	Unsolved	14.0	4.0	2.0
	P'	8.5	9	17.0	"	12.0	1.25	.5
	Q'	5.0	1	31.0	Incorrect	10.0	4.0	2.25
	W'	6.5	0	29.0	Unsolved	18.0	Unsolved	
	I'	6.0	0	78.0	Incorrect	29.0	4.0	1.25
	N'	10.0	0	26.0	Unsolved	10.0	3.0	1.5
	Av.	8.2		40.2		18.0	2.5	1.06
	S.D.	2.9		33.8		7.9	1.9	
Groups	A'	3.0	0	28.0	Incorrect	3.0	.25	.5
	B'	12.0	0	45.0	"	10.0	4.0	2.25
	C'	6.0	1	69.0	"	00.0	Unsolved	
	D'	2.0	0	26.0	"	8.0	5.0	1.50
	E'	4.5	0	40.0	"	10.0	1.25	.75
	Av.	5.5		41.6		8.0	2.6	1.25
	S.D.	3.5		15.4		2.9		

None of the individual *S*s solved Problem 2 correctly; 11 stated that they could not solve it and presented no solution. The 6 other individual solutions varied in their degree of imperfection. One *S* simply composed a complete sentence to finish the sonnet. Three others had either 3 or 4 of the rhyme words, but an erroneous rhyme scheme. One person had the first line in accord with the original and one had the last line correct. Others made more or less serious errors here. The second and third lines were never correctly written by any individual. All groups turned in a solution. The rhyme

scheme was correct in all cases; the first line was correct in 3 cases and the last one in 4. Group B had the two middle lines almost correct. The meaning was the same as that in the original, although several words were misplaced. In some group and some individual solutions the words had been so arranged as to convey a meaning almost opposite to that in the original.

Neither a group nor an individual arrived at a correct solution of the third problem. The average error in location and average excess number of miles traveled were about equivalent for the groups and individuals. In part, the absence of a correct solution may be due to the fact that it took a long time to solve the first and second problems. In many cases the completion of the third meant dismissal for the evening; thus perhaps any apparently suitable location was accepted. (On the other hand many persons took the problems home with them in order to complete the second and third. They kept time for themselves and returned the solutions the next week.) A comparison of Tables III and IV shows that among the groups the total number of suggestions was by far the least in the third problem.

Reference to the notes kept by the note-takers will give more definite information regarding the activities within the groups. All those in a group do not participate equally in the group activity. Such remarks or tabulations as the following are found in the notes from three groups in all. (The note-takers numbered the *S*s to facilitate taking notes.) "*S* 1 and *S* 3 were leaders in the solution." From the tabulation of suggestions given in the solution referred to it is found that *S* 1 made 7 suggestions and *S* 3 made 14 suggestions; while *S* 2 and *S* 4 made, respectively, 2 and 3 suggestions. Later in these same notes we find, "*S* 2 not contributing much." In another set of notes: "*S* 3 and *S* 4 offered no suggestions during the solution." In the next problem solved by this same group *S* 2 made 16 suggestions, *S* 1 made 12, *S* 4 made 8, and *S* 3 made 5. From yet another set: "*S* 1 and *S* 4 did most of the suggesting, *S* 2 and *S* 3 not working much." In solving Problem 2 in the first half of the experiment the note-taker remarks that "*S* 2 and *S* 3 draw their own diagrams and become absorbed in them: do almost no suggesting." In solving Problem 3 "*S* 3 made no comments at all, *S* 2 spoke only a few words." In one of the above groups an *S* who participated a great deal in group activity very soon assumed the task of manipulating the material in the place of the less active

S who had been appointed chairman. Possibly one could get interesting, meaningful, and perhaps quite different sets of results by using in one set of cases groups with a chairman either equal or superior to the group, and in another set groups having a chairman inferior to the group in ability.

In some groups quite the reverse situation is found, and all members coöperate splendidly. Such comments as the following are found in the notes from the three groups: "All contributing beautifully," and later, "all coöperating and making check suggestions." From another, "suggestions coming from all four about equally." Another records, "the four members coöperate well," and on another page, "splendid group work."

TABLE III

SHOWING, FOR EVERY GROUP OF *S*S, THE NUMBER OF CORRECT AND INCORRECT SUGGESTIONS AND REJECTIONS, AND THE NUMBER OF REJECTIONS BY THOSE MAKING THE SUGGESTIONS AND BY OTHERS OF THE GROUP
(Problem 1, Second Half of Experiment)

	Suggestions			Rejections			
Groups	No.	Correct	Incorrect	Correct	Incorrect	Maker	Others
A′	29	18	11	3	11	7	7
B′	24	18	6	1	6	1	6
C′	17	8	9	0	8	0	8
D′	20	11	9	2	9	2	9
E′	31	25	6	2	6	2	6
Totals	121	80	41	8	40	12	36

In the first half of the experiment Groups A and E solved all problems incorrectly. From notes kept there was apparently but little criticism of the work in Group E. The note-taker remarks, "All satisfied with the solution." The members of Group A, however, seemed to recognize that they were not taking all specifications of the problem into consideration, but rationalized their procedure and turned in solutions. At the end of the first problem the recorder notes that "they conclude that they have solved the problem, though perhaps not in the way the directions signify." This group is one of those referred to above in which the members do not participate equally in the group activity. In Problem 2 they finally agreed that the boat's contact with the shore would not constitute a case of outnumbering by the cannibals! In Problem 3 they failed to consider circle B always as a transfer station, but skipped it whenever it was convenient to do so.

In the other groups there is much more reference to the checking of errors and meeting the conditions of the problem. One group worked a solution through three times to be sure that they had met all specifications; in the last trial they discovered an error which would have made their solution wrong. The notes on these groups also mention numerous references to the stated problem to see that all qualifications were being taken into account.

Table III above deals with Problem 1 in the second half of the experiment. Column 2 shows the total number of suggestions made in each group. Columns 3 and 4 show respectively the number which were in reality correct and the number which were incorrect. Columns 5 and 6 indicate whether those suggestions which were rejected were respectively correct or incorrect. Columns 7 and 8 show the number of suggestions which were rejected by the individual making the suggestion or by another member of the group.

It will be noted from Table II that there was only one error (a word transposition) in any group solution. In one case, Group A′, the suggestions rejected by the proposer or by another are exactly equal in number; in all other cases more were rejected by another member. Considering all groups together, three times as many suggestions were rejected by another member of the group as by the proposers of the suggestions. Five times as many incorrect as correct suggestions were rejected, whereas of the total number of suggestions made, twice as many were correct as incorrect. This fact may be considered in connection with the relative number of correct solutions among groups and among individuals. This quantitative check on rejections was not kept in the first half of the experiment, but from its results in the second half, together with the proportion of correct solutions in the first half, and the fact that notes on the groups presenting correct solutions emphasize the checking of erroneous moves, it seems as though group supremacy in the first half might have been in part due to the rejection of incorrect suggestions or the checking of errors. Also it was found that no group was composed of four superior individuals. All this would seem to indicate that one point of group supremacy is the rejection of incorrect ideas that escape the notice of the individual when working alone. Perhaps this may be the greatest point of group supremacy.

It is impossible to say with any certainty whether the rejected correct suggestions were rejected oftener by the proposer or by another member of the group. Only 8 correct suggestions were

rejected, and these were all later accepted, since the solutions were with but one exception absolutely correct. (In the solution where two words were transposed, the correct suggestion concerning their position was never made: this was a case of the acceptance and retention of an incorrect suggestion.) Five of the correct suggestions were rejected by the proposer and 3 by another, but with so small a total the difference is not significant.

TABLE IV

SHOWING, FOR EVERY GROUP OF *Ss*, THE NUMBER OF CORRECT AND INCORRECT SUGGESTIONS AND REJECTIONS, AND THE NUMBER OF REJECTIONS BY THOSE MAKING THE SUGGESTIONS AND BY OTHERS OF THE GROUP
(Problem 2, Second Half of Experiment)

	Suggestions			Rejections			
Groups	No.	Correct	Incorrect	Correct	Incorrect	Maker	Others
A′	71	32	39	10	35	18	27
B′	49	23	26	9	20	8	19
C′	76	35	41	9	29	7	31
D′	32	15	17	2	13	4	11
E′	37	17	20	2	14	3	13
Totals	265	122	143	32	111	40	101

TABLE V

SHOWING, FOR EVERY GROUP OF *Ss*, THE NUMBER OF CORRECT AND INCORRECT SUGGESTIONS AND REJECTIONS, AND THE NUMBER OF REJECTIONS BY THOSE MAKING THE SUGGESTIONS AND BY OTHERS OF THE GROUP
(Problem 3, Second Half of Experiment)

	Suggestions			Rejections			
Groups	No.	Correct	Incorrect	Correct	Incorrect	Maker	Others
A′	17	6	11	3	7	4	6
B′	10	3	7	1	6	0	7
C′	(No solution attempted)						
D′	10	2	8	0	5	0	5
E′	13	5	8	2	4	1	5
Totals	50	16	34	6	22	5	23

Tables IV and V summarize the data received on Problems 2 and 3. Since neither of these problems was correctly solved in any case, the value of the group checking does not appear so clearly, But a consideration of these results, with an analysis of individual and group work on these two problems, brings out the same fact as above. For example, only 6, or 35%, of the individual *Ss* presented a solution to Problem 2, as compared with 5, or 100%, of the groups. But perhaps more important than this is the fact that only one individual solution presented the idea conveyed by the part of the sonnet quoted; the other presented an opposite situation. (That is, they had *Simple Simon* railing "at

ill luck and unkind fate.") Three groups conveyed the correct idea, while only 2 reversed the conditions. That is, only 1/6 of the individual *Ss*, as compared with 3/5 of the groups, succeeded in grasping and presenting the proper situation. It seems not altogether improbable that this is a direct result of the rejection of incorrect ideas in the group; which, it should be noted again, is done largely by another member than the proposing one (2.52 times as many suggestions were rejected by another as by the proposer).

We find in the case of Problem 3, first, that all groups met the requirement as to the capacity of the buses, whereas 2 individual *Ss* placed more than 35 pupils in a bus (one placed 43 and the other 37 in one of the buses). Other than this, however, no superiority either of group over individual or of individual over group is shown when the two are compared as a whole.

Summary

The purpose of the present study was to compare the ability of individuals and coöperating groups of 4 persons in solving complex problems. The problems involved a number of steps, all of which had to be correct before the right answer was obtained, but they are still far from the life-situations usually met. The groups were roughly equated so that no one group was composed of 4 superior individuals, but the students used were a highly selected group when compared with the population as a whole.

Upon the basis of the data and discussion presented in the foregoing pages the following conclusions seem justified:

(1) Groups seem assured of a much larger proportion of correct solutions than individuals do.

(2) This seems to be due to the rejection of incorrect suggestions and the checking of errors in the group.

(3) In groups of the size here used more incorrect suggestions are rejected by another member of the group than by the individual who proposed the suggestion.

(4) All members do not coöperate or participate equally in the solution of the problems.

(5) In erroneous solutions (where it is possible to determine the exact point at which the first error was made), groups do not err so soon as the average individual does.

TIME RELATIONS FOR THE DIFFERENT CATEGORIES OF JUDGMENT IN THE 'ABSOLUTE METHOD' IN PSYCHOPHYSICS

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The question of the time relations of judgments of different categories in psychophysics is a problem on which a good deal has been written but one on which surprisingly little quantitative experimental work has appeared. Kellogg,[1] in his historical review of the literature, mentions only 7 studies, most of which do not apply directly to the present problem.

Martin and Müller,[2] and George[3] discovered that the judgments of 'doubtful,' or undecided and the like, took longer than the formation of other categories of judgment and concluded that they should be excluded from the psychophysical measurement methods. George also found that the 'equal' judgment required more time than those of 'less' or 'greater,' the order and average magnitude of the reaction times being: 'less' = 0.88 sec.; 'greater' = 0.92 sec.; and 'equal' = 1.10 sec.

In Kellogg's own study of visual intensities, two arrangements were employed: a two-category plan, in which only judgments of 'plus' and 'minus' were admitted; and a three-category plan, in which 'equal' judgments were added to the other two categories. For each of 5 *Os* the average of the reaction-times of the two-category plan were shorter than those for the three-category plan. Kellogg concluded that "the judgment times in the three-category series average throughout about 10% larger than those in the two-category series. The increase in time is explained as a function of the greater subjective difficulty of being prepared for three possible alternatives in the one case as against the relatively simpler preparation of two

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[1] W. N. Kellogg, The time of judgment in psychometric measures, this Journal, 43, 1931, 65-86.

[2] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 196-206.

[3] S. S. George, Attitude in relation to the psychophysical judgment, this Journal, 28, 1917, 1-37.

alternatives in the other."[4] Unfortunately for our present purpose, Kellogg in no place gives the reaction-times for each of the categories in his three-category plan.

There can be no doubt that the 'equality' judgment has been open to suspicion since the beginning of psychophysics. Much of this suspicion has a purely introspective basis, the 'equal' judgments seemed less positive, slower, and more frequently required confirmation than the judgments of difference.[5] Martin and Müller's and George's experimental findings seem to definitely confirm the introspective findings with regard to the slower relative speed of the formation of the 'equal' judgment.

Some time ago the senior author presented results which indicated the possibility of employing the 'absolute method' in psychophysics.[6] The classical method has been to present to the *O*s stimuli in pairs—a standard stimulus and a comparison stimulus. In the 'absolute method' only single stimuli are presented to the *O* and for each stimulus he is instructed to give a judgment in terms of the three categories, 'less,' 'intermediate,' and 'heavy.' It was found that if completely naïve *O*s are given such an absolute series normal curves of the psychometric functions are obtained for all three categories as the basis of the calculation of limens, although there is less precision of judgment and greater intervals of uncertainty than one finds in the usual relative series. These findings were confirmed by Davies and Babb.[7]

It seemed worth while to determine the reaction-time relations of the formation of judgments of the three categories of judgment by the 'absolute method' to discover whether there is any relation between the time relations for the absolute method and those which had been previously found for the three-category plan for the 'relative method.' The relatively larger number of 'intermediate' judgments for the absolute method as compared with the number of 'equal' judgments for the relative method (as evidenced by the greater magnitude of the interval of uncertainty) seemed a favorable condition for the present study. The importance of the present study

[4]Kellogg, *op. cit.*, 85.

[5]For a detailed review of the literature concerning the equality judgment, cf. S. W. Fernberger, The use of equality judgments in psychophysical procedures, *Psychol. Rev.*, 37, 1930, 107-112.

[6]Fernberger, On absolute and relative judgments in lifted weight experiments, this JOURNAL, 43, 1931, 560-578.

[7]E. Davies and W. W. M. Babb, Concerning the absolute method in psychophysics, this JOURNAL, 44, 1932, 367-369.

has its basis in the acceptance of the criterion of reaction-time difference as the basis for the retention or elimination of categories of judgment in the hands of both George and of Kellogg.

METHOD

Lifted weights were employed in the present study. Fourteen aluminum cylinders were cast, and drilled and filed to the proper weight. They varied, in the regular series, from 88 to 112 grm. by intervals of 4 grm. These stimulus-objects were placed about the circumference of a circular table with a revolving top so that each one could be successively brought under the reacting hand of *O*.

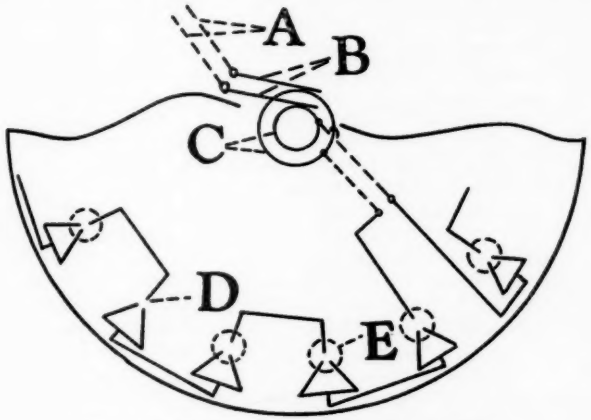

FIG. 1. DETAIL OF WIRING OF STIMULUS-TABLE
A, wires to chronoscope; B, brushes beneath table; C, collection rings beneath table; D, gap in wiring; and E, gap in wiring closed with weight in position.

Two series were employed, each carefully arranged in order to reduce the effects of interserial expectation. The order was changed frequently in order to eliminate the possibility of learning the series.[8]

These metal weights rested on copper wires fastened to the surface of the table and these wires were connected with a Dunlap Chronoscope.[9] The wiring arrangement was such that the lifting of any weight opened a gap which the weight bridged when it was on the table and so actuated the chronoscope. After considerable experimentation a bridge contact of the form shown in Fig. 1 was adopted. The wiring will be observed to be discontinuous, forming a gap which corresponded to the position on the table of every weight so that the weights

[8]Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-150.

[9]This experiment was made possible by a grant from the Faculty Research Committee of the University of Pennsylvania for the purchase of the Dunlap chronoscope and accessory apparatus.

closed the gap made by the three wires. The current passed through collection rings underneath the table. In order that the top of the table might swing freely it was necessary to take the current to and from the collection rings by means of brushes and these were wired directly to the chronoscope. Hence the circuit was closed only when all of the weights covered the gaps and the lifting of any one of the 14 weights would break the circuit and actuate the chronoscope.

The advantage of this three-point contact is that any weight may be moved about the surface of the table to a considerable extent and still rest on two of the three essential wires. We first tried a series of parallel wires but found that a relatively small displacement of the stimulus led to a tilting of the weight which caused a poor contact. Substitution of nickel plated parallel brass bars for the parallel wires gave no better contact and then the three-point contact described above was adopted.

Thus the chronoscope was started automatically by the lifting of a weight and it was hoped that the chronoscope might be stopped by having the judgment verbalized into a voice key. Preliminary experiments indicated, however, that this was not feasible due to the fact that the *Os*, in a long series of judgments, characteristically allow the head to drop so that the chin rests on the chest and so they fail to maintain a constant spatial relation between the mouth and the voice key. Furthermore it might be suspected, in the absence of either positive or negative evidence, that there might be differences in actuation-time of the voice key by the different words of the various categories of judgments and, if this were true, it would be the basis of a constant error. We therefore discarded the voice key and, throughout the experiments here reported, employed a pneumatic key actuated by pressing a rubber bulb for stopping the chronoscope.

The constant speed motor of the Dunlap chronoscope was run on commercial A.C. 60 cycle current. This current proved to be of such cycle constancy as to be within the limits of sensitivity of the chronoscope.[10] Thus, running on this current each unit of the chronoscope dial had a time value of 1/1200 sec.

The weights were presented successively to the *Os* except as noted below.[11] In order to allow time for the reading of the chronoscope it was not possible to have the lifting done continuously and controlled by the beats of a metronome in the usual manner. In the present experiment, each lifting was a disparate affair. *E* said "Ready, now, one, two, three, four" at a rate maintained as accurately as possible at 92 per min. On the signal "Ready, now" *O* raised his hand; on "one" the hand was lowered and the weight grasped; on "two" the weight was lifted; on "three" the weight was replaced on the table and on "four" the empty hand was lifted.

The *Os* were given the following instructions at the beginning of each experimental period:

> You will be presented with a series of weights which vary only slightly one from another. Some are light, some are heavy, and some are intermediate. Immediately after the lifting of a weight, you will report whether it was light, intermediate, or heavy. The form of your report will consist primarily of the

[10] Fernberger, The accuracy of phase cycles in a commercial electric current, this JOURNAL, 44, 1932, 582-583.

[11] For a detailed description of the manner of lifting the weights cf. S. W. Fernberger, On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 13, 1913 (no. 61), 6-11.

pressing of the bulb. It is immaterial whether or not you verbalize your judgment simultaneously with the pressing of the bulb or subsequently.

In spite of the last part of these instructions, however, every *O* adopted the method of verbalizing the judgment simultaneously with the pressing of the bulb.

Four *O*s, all graduate students in psychology at the University of Pennsylvania, completed a long series of judgments during the autumn and winter of the academic year, 1931-1932. Those who took part were Mr. Leon Arons (*A*), Mr. Emil E. E. Folgmann (*F*), Mr. Winslow N. Hallett (*H*), and Mr. Malcolm G. Preston (*P*). *A* and *H* were highly practiced in lifted-weight procedures, and *F* and *P* were entirely without practice. None of the *O*s had practice in the formation of judgments by the absolute method before the present experiment.

A total of 12,950 judgments were obtained in the entire experiment. For *F*, *H*, and *P* 10 complete series of 350 judgments each were obtained. For *A* only 7 complete series of 350 judgments were obtained. At the end of the seventh series *A* suffered a prolonged illness and it seemed inadvisable to complete his data after the long interval.

The judgments and the reaction-times were both recorded in such a way that complete identification and correlation of the two sets of data was possible. The senior author throughout the experiment presented the weights to the *O*s and recorded the judgments and the junior author throughout the experiment read the chronoscope.

The chronoscope was read throughout to the exact unit of the dial. For purposes of calculation the reaction-time readings were treated by grouping to the nearest 10. The calculations were made in terms of the units of 1/1200 sec. and were transposed to sigma units only at the end of the calculations. The limens were treated in accordance with Urban's treatment of the results for the method of constant stimuli.

Results

Limens. Although our primary interest in the present study is in the reaction-times for the formation of the judgments of the different categories, it seems worth while to include a discussion of the limens and other significant values of the psychometric functions for the sake of completeness because we have still so little information regarding the absolute method. These values will be found in Table I. In the first two columns will be found the values of the indices of precision (h_1 and h_2) for the curves of the psychometric functions of the light and heavy judgments respectively. In the next two columns will be found the values of S_1 and S_2, the lower and upper limens respectively. In the next column are entered the values of the intervals of uncertainty (I. of U.) and in the last column the points of symmetry (P. of S.).[12] In each row will be found the

[12]We have, as the result of correspondence with Professor E. G. Boring, adopted this term for that value of stimulus corresponding to the intersection of the curves of the psychometric function of the 'less' and 'great' judgments. The older term, "point of subjective equality," is not, of course, applicable when a standard stimulus is not used.

TABLE I

SHOWING FOR EVERY *O* AND FOR EVERY SERIES OF EXPERIMENTS THE UPPER AND LOWER LIMENS, THE INDICES OF PRECISION, THE INTERVAL OF UNCERTAINTY, AND THE POINT OF SYMMETRY

O	Series	h_1	h_2	S_1	S_2	I. of U.	P. of S.
	1	0.097	0.104	93.54	101.25	7.71	97.54
	2	0.086	0.096	93.49	102.15	8.66	98.05
	3	0.056	0.086	92.00	102.16	10.16	98.18
A	4	0.079	0.070	90.84	99.98	9.14	95.14
	5	0.088	0.115	90.16	99.40	9.24	95.39
	6	0.136	0.102	91.18	98.54	7.36	94.34
	7	0.100	0.094	93.25	102.16	8.91	97.55
	Av.	0.092	0.095	92.07	100.81	8.74	96.60
	1	0.109	0.076	91.04	103.26	12.22	96.06
	2	0.095	0.087	90.16	103.09	12.93	96.32
	3	0.117	0.076	92.91	106.67	13.76	98.33
	4	0.105	0.093	93.03	193.79	10.76	98.10
	5	0.095	0.094	93.27	105.24	11.97	99.21
F	6	0.054	0.066	90.78	106.86	16.08	99.62
	7	0.086	0.095	92.21	105.01	12.80	98.95
	8	0.091	0.094	89.26	103.89	14.63	96.66
	9	0.101	0.072	91.70	105.43	13.73	97.40
	10	0.103	0.066	91.51	194.86	13.35	96.74
	Av.	0.096	0.082	91.59	104.81	13.22	97.74
	1	0.073	0.058	92.48	103.03	10.55	97.19
	2	0.090	0.087	93.08	102.34	9.26	97.62
	3	0.048	0.071	88.57	103.43	14.86	97.38
	4	0.060	0.065	90.97	103.21	12.24	97.36
	5	0.092	0.060	93.13	105.60	12.47	98.02
H	6	0.098	0.080	92.63	104.55	11.92	97.98
	7	0.079	0.081	91.98	104.09	12.11	98.07
	8	0.092	0.076	93.59	104.19	10.60	98.38
	9	0.083	0.092	94.76	105.05	10.29	99.83
	10	0.068	0.086	94.75	107.57	12.82	101.89
	Av.	0.078	0.076	92.59	104.31	11.71	98.37
	1	0.087	0.092	90.64	102.16	11.52	96.56
	2	0.122	0.104	92.24	104.39	12.15	97.84
	3	0.091	0.110	92.13	106.57	14.44	100.03
	4	0.065	0.077	87.21	104.96	17.75	96.85
	5	0.080	0.082	91.80	107.84	16.04	99.92
P	6	0.089	0.070	96.67	111.74	16.07	102.75
	7	0.087	0.099	95.23	109.18	13.95	102.65
	8	0.123	0.191	96.28	107.47	11.19	103.09
	9	0.109	0.178	99.33	107.43	8.10	104.36
	10	0.123	0.149	98.45	108.84	10.39	104.14
	Av.	0.098	0.115	93.90	107.58	13.16	100.82

values for each fractionated series in the order in which they were taken and in the last rows will be found the average values for the entire experiment.

The magnitudes of all of these significant values correspond to those found by Fernberger[13] and by Davies and Babb.[14] The intervals of uncertainty are invariably larger than those obtained by the relative method. For *A*, *F*, and *H* the values of the point of symmetry are lower than the central value of the stimulus-series (except for the value for Series 10 of *H*). An anomalous situation is found in this respect for *P*. For him 6 of the 10 values of the point of symmetry (and the average for the entire experiment) are higher

TABLE II

SHOWING FOR EVERY *O* THE FREQUENCY OF OCCURRENCE OF THE DIFFERENT CATEGORIES OF JUDGMENT

O	Light	Intermediate	Heavy
A	618	672	1160
F	816	1466	1218
H	973	1238	1289
P	1083	1488	929

than the middle stimulus of the series employed. A study of the values for the point of symmetry of this *O* indicates a shift from a negative to a positive error as the result of progressive practice. We have no ready explanation to offer for this anomalous situation with regard to the error of the point of symmetry for *P*.

Reaction-times. In Table II will be found the distribution of the frequencies with which the judgments of the different categories were given by each of our *O*s. It will be noted that these frequencies vary among them. *A*, *F*, and *H* give more judgments of heavy than light—a fact evidenced by the negative error in the points of symmetry discussed above. Also, as one would expect from the positive error of the point of symmetry, *P* reported more judgments of light than of heavy. One finds that *F* and *P* gave more judgments of intermediate than they did for either of the extreme categories, while *A* and *H* gave more intermediate judgments than they did judgments of light but not so many as of heavy.

We first calculated the reaction-times for the entire experimental data for each *O*. These results will be found in Table III. Two rows are given to the results of each *O*. In the first row, in the

[13]Fernberger, *op. cit.*, this JOURNAL, 43, 1931, 560-578.
[14]Davies and Babb, *op. cit.*, 367-369.

appropriate column, will be found the average reaction-times for the three categories of judgment, while in the same columns in the row immediately beneath will be found the probable errors of these averages.

It will be observed that the average reaction-times of the intermediate judgments for *F*, *H*, and *P* are larger than for those of either light or heavy judgments. For *A* the average reaction-time for intermediate is very slightly longer than that for light and equal to that for the heavy judgments. It will also be observed that the reaction-times for judgments of light and heavy are very similar for

TABLE III

SHOWING FOR EVERY *O* THE AVERAGE REACTION-TIME AND P.E. FOR EVERY ONE OF THE DIFFERENT CATEGORIES OF JUDGMENT

(Times in sigma)

O		Light	Intermediate	Heavy
A	Av.	328	334	334
	P.E.	95	84	86
F	Av.	864	909	860
	P.E.	156	153	163
H	Av.	609	673	639
	P.E.	120	140	122
P	Av.	1510	1545	1520
	P.E.	104	118	122

all the *O*s. For *A*, *H*, and *P* the averages for the light judgments are smaller than those for the heavy, while for *F* the reaction-times for heavy are somewhat smaller than for the light judgments.

The other marked characteristic of this table is the large magnitude of the probable errors throughout for every *O* and for all of the categories of judgment. There seems to be no regularity of the order of magnitude of the probable errors for the different *O*s. For *A* the order of the probable errors from smallest to largest is intermediate, heavy, and light; for *H* it is light, heavy, and intermediate; for *F* it is intermediate, light, and heavy; and for *P* it is light, intermediate, and heavy. Each of the probable errors for each of the three categories of judgment are thus found sometimes as lowest, sometimes as highest, and sometimes in an intermediate position. In no case are the differences in the size of the probable errors for the three categories for any *O* other than relatively small.

The differences between the average reaction-times are given in Table IV. For *A* the difference in average reaction-times are very small throughout. For *F* there is a small difference between the times for the light and heavy judgments and much larger and almost equal differences between the reaction-times for the light-intermediate and heavy-intermediate comparisons. For *H* the differences are larger on the whole than those for the other *O*s. The difference between the times for light and heavy and for heavy and intermediate are of similar magnitude and the difference between the times

TABLE IV

SHOWING FOR EVERY *O* THE DIFFERENCES AMONG THE REACTION-TIMES OF THE VARIOUS CATEGORIES OF JUDGMENT

O	Light Heavy	Light Intermediate	Heavy Intermediate
A	6	6	0
F	4	45	49
H	30	64	34
P	10	35	25

TABLE V

SHOWING THE SIGNIFICANCE OF THE DIFFERENCES GIVEN IN TABLE IV

O	Light Heavy	Light Intermediate	Heavy Intermediate
A	0.027	0.027	0.000
F	0.011	0.113	0.118
H	0.171	0.187	0.097
P	0.118	0.032	0.097

for light and intermediate is about twice that of the other two comparisons. For *P* the order from smallest to largest difference in reaction times is light-heavy, heavy-intermediate, and light-intermediate.

In general, then, the differences in reaction-times are smallest for the light-heavy comparison and larger for the light-intermediate and heavy-intermediate comparisons but the relation is not as clear cut as one might expect from analogy with the results of George and of Kellogg.

It seems worth while to apply some criterion of the significance of a difference and for this purpose we have chosen the values of P_z the values for which will be found in Table V. These values give the probability that the difference will not vary by an amount greater than itself and they may therefore be considered as an index of

significance of the difference. They are dependent upon a ratio between the difference and the probable error of the difference and are found in a special table in Johnson.[15] In the first column of this table are given the values of P_z for the difference between the average reaction-times of light and heavy judgments for each *O* for the entire set of experimental data and in the next two columns, respectively, the values of P_z for the differences between the judgments of light and intermediate and of heavy and intermediate. It will be observed that on the basis of this criterion none of the differences are at all statistically significant. Indeed, a closer examination of the table indicates that the differences between the light-heavy reaction-times are no more significant than those between light-intermediate or heavy-intermediate.

It has seemed worth while to fractionate the results in terms of each series of 350 judgments in the order in which they were taken, and to calculate the averages and probable errors for each fraction and for every *O*. These values will be found in Table VI.

An examination of Table VI indicates, for *A*, that there is a tendency for the average reaction-times to decrease rapidly in size at the beginning of the experiment and then more slowly for all of the three categories of judgment. Also there is a very marked decrease in the size of the probable errors from the first to all subsequent series. After the first series the probable errors for this *A* remain very constant except for a more gradual decline through the second series for the heavy judgments. It will be remembered that *A* was highly practiced in lifted-weight procedure before the beginning of the present experiment.

The results for *F* are on the whole similar but show, in some respects, marked difference from those of *A*. Decreases in the size of the average reaction-times and in the size of the probable errors are found throughout. But the decreases in the size of the averages are much more marked for *F*—the averages for the last series being approximately one-half of those for the first series. Also the decreases in the reaction-times although rapid at first and slower later, continue to be observable throughout the entire experiment. One is led to believe that for *F* further experimentation would have led to further decrease in reaction-times, while in the case of *A* the reaction-times seem to be entirely stabilized long before the end of the experiment.

[15] W. W. Johnson, *The Theory of Errors and Method of Least Squares*, 1892, Table II, 154.

TABLE VI

SHOWING FOR EVERY *O* THE AVERAGE REACTION-TIME AND P.E. FOR EVERY SERIES OF EXPERIMENTS FOR EVERY ONE OF THE DIFFERENT CATEGORIES OF JUDGMENT

(Time in sigma)

		Light		Intermediate		Heavy	
O	Series	Av.	P.E.	Av.	P.E.	Av.	P.E.
	1	528	152	523	137	527	128
	2	306	38	343	48	347	82
	3	330	45	367	45	362	48
A	4	242	40	256	36	259	38
	5	302	58	306	36	305	39
	6	292	32	288	33	294	32
	7	260	41	262	36	267	34
	1	1362	175	1398	182	1338	153
	2	1032	75	1069	73	1007	85
	3	920	88	970	79	898	90
	4	864	67	918	68	881	89
F	5	862	77	942	62	865	75
	6	782	70	858	76	764	68
	7	709	81	761	73	698	76
	8	754	59	768	45	706	51
	9	713	72	766	46	683	59
	10	655	69	700	70	638	68
	1	892	150	1018	162	909	130
	2	741	99	862	104	746	91
	3	712	78	805	73	716	74
	4	650	67	729	89	683	84
H	5	613	82	681	93	632	90
	6	528	62	578	72	550	68
	7	512	47	520	59	512	52
	8	479	68	528	60	467	51
	9	499	72	558	73	533	70
	10	512	49	548	68	533	65
	1	1379	142	1318	119	1314	121
	2	1687	101	1688	90	1658	88
	3	1715	71	1728	92	1722	88
	4	1601	103	1652	96	1607	88
P	5	1615	69	1607	84	1546	74
	6	1450	69	1472	81	1414	72
	7	1398	71	1437	70	1420	83
	8	1442	58	1500	58	1539	68
	9	1453	49	1452	51	1456	48
	10	1488	52	1488	58	1498	48

For *F* there is a marked decrease in the size of the probable errors which, however, seems to have reached a low level by the end of the experiment.

The results for *H*, an *O* also trained in lifted-weight technique, conform rather well with those of *F*. There is a decrease in the size of the average reaction-times and of the probable errors for all three categories of judgment. These decreases are great at first, less later, and seem to continue well toward the end of the experiment, where the values tend to level off and become stabilized at about the Series VII.

The results for *P* more or less conform to those of the other 3 *O*s. *P* was entirely untrained in lifted-weight technique at the beginning of the present experiment. His results indicate a marked reduction in the sizes of the probable errors, relatively rapid at first and then proceeding more slowly. If one should eliminate the average reaction-times for the first two series, the results for *P* in the last eight series entirely and completely confirm those of the other *O*s. For the last eight series, the average reaction-times start relatively high and at first rapidly and then more slowly decrease in magnitude. The reaction-times for all three categories of the first series, however, are smaller for *P* than those for any subsequent series. For Series 2 they are all considerably higher but not so large as the reaction-time for Series 3. We deemed it unwise to question this *O* during the experiment as to the possible basis of this anomalous result. Questioning after the experiment was completed elicited no positive explanation. We suggested the following explanation to this *O* to which he agreed but without any high degree of assurance: at the beginning he approached the experiment with a naïve stimulus attitude which tended to give relatively rapid reaction-times for the formation of his judgments and that, after the first day's experimentation, he assumed a more critical attitude which tended to increase the length of the reaction-times. As a bit of confirmatory evidence, we noted that in Series 2 *P* volunteered the information that "the formation of these judgments is not as easy as I thought."

Except for the anomalous first two series the rest of *P*'s results and all of the results for the other *O*s are in substantial conformity. The reaction-times are relatively slow at first and, with progressive practice, decrease rapidly in size and then more slowly. For all *O*s the probable errors are at first large and, with progressive practice, drop at first rapidly and then more slowly. The initial drop for the

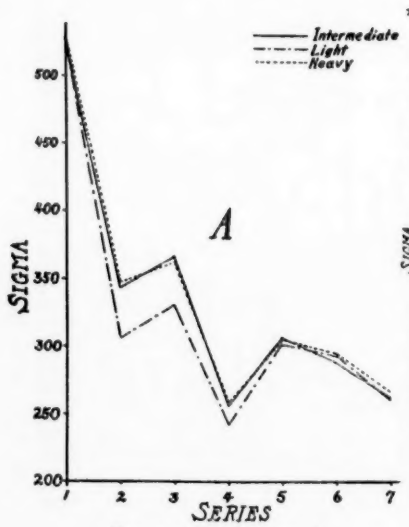

FIG. 2. CURVES FOR *A*.

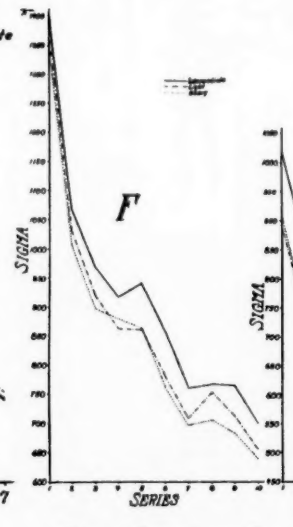

FIG. 3. CURVES FOR *F*.

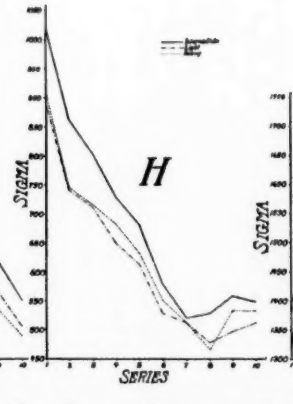

FIG. 4. CURVES FOR *H*.

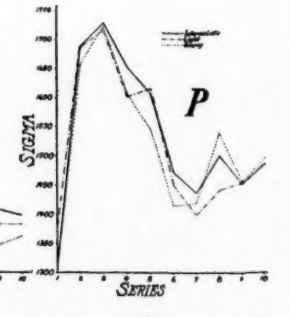

FIG. 5. CURVES FOR *P*.

probable errors is more rapid than that for the reaction-times and the values of the probable errors stabilize at a relatively low level earlier in the experimental series.

For the convenience of comparing the relations of the reaction-times of the different categories of judgment for the fractionated series, we have thrown the reaction-time results of Table VI into the form of curves shown in Figs. 2–5. The curves for each *O* are given in separate figures. It will be observed that the curves for all the *O*s are relatively of the parabolic form typical of one type of the usual learning curve, except for the anomalous results of Series 1 and 2 for *P*. The relative steepness of the curves for the different *O*s, discussed above, becomes more evident in this graphic form.

The primary problem of the present study, however, concerns the relative size of the reaction-times for the different categories of judgment, and, as it turns out, at different stages in progressive practice.

For *A* (Fig. 2) all three curves are almost identical throughout. For all 7 fractions, the curves for intermediate and heavy judgments may be considered identical. In two fractions, Series 1 and 2, the reaction-times for light judgments are slightly smaller than those for the other two categories. For the other 5 fractions, Series 3–7, the curve for light judgments very closely corresponds to those of the other two categories.

For *F* (Fig. 3) the curves are very similar in form and throughout are parallel to one another. The reaction-times for the intermediate judgments are slowest for every fraction. The times for the light and heavy judgments are very similar. In 8 of the 10 fractions, the reaction-times for heavy are slightly smaller than those for the light judgments.

For *H* (Fig. 4) it is evident that the reaction-times for intermediate judgments are invariably larger than those for the other two categories for all 10 fractions. All three curves are, however, very similar in form and are relatively parallel throughout. There is especially great similarity between the curves for the light and heavy judgments. In 8 of the 10 fractions the curve for the light judgments is lower than that for the heavy judgments.

The curves for *P* (Fig. 5) are again all very similar for all categories of judgment. The different curves cross one another so frequently that we are unable to note any special tendencies as in the cases of the curves for *F* and *H*. One must conclude that the curves for *P* are similar to those of *A* in that examination discloses no constant differences of any sort.

TABLE VII

SHOWING FOR EVERY *O* THE DIFFERENCES AMONG THE REACTION-TIMES OF THE VARIOUS CATEGORIES OF JUDGMENT FOR EVERY SERIES OF EXPERIMENTS AND THE SIGNIFICANCE OF THOSE DIFFERENCES

		Light-Heavy		Light-Intermediate		Heavy-Intermediate	
O	Series	D	P_z	D	P_z	D	P_z
	1	1	0.000	5	0.011	4	0.091
	2	41	0.238	37	0.319	4	0.022
	3	32	0.254	37	0.304	5	0.043
A	4	17	0.160	14	0.134	3	0.032
	5	3	0.027	4	0.032	1	0.011
	6	2	0.032	4	0.038	6	0.070
	7	7	0.070	2	0.048	5	0.016
	1	24	0.954	36	0.081	60	0.123
	2	25	0.123	37	0.187	62	0.294
	3	22	0.091	50	0.223	72	0.314
	4	17	0.081	54	0.294	37	0.176
F	5	3	0.016	80	0.415	77	0.406
	6	18	0.102	76	0.378	94	0.478
	7	11	0.059	53	0.254	63	0.314
	8	48	0.324	14	0.102	62	0.465
	9	30	0.171	53	0.324	83	0.542
	10	17	0.097	45	0.244	62	0.334
	1	17	0.043	126	0.299	109	0.274
	2	5	0.016	121	0.434	116	0.429
	3	4	0.016	93	0.438	89	0.434
	4	33	0.166	79	0.368	46	0.197
H	5	19	0.086	68	0.284	49	0.197
	6	22	0.129	50	0.279	28	0.155
	7	0	0.005	8	0.059	8	0.054
	8	12	0.081	49	0.284	61	0.368
	9	36	0.197	61	0.309	25	0.123
	10	21	0.144	36	0.233	15	0.081
	1	65	0.187	61	0.176	4	0.011
	2	29	0.118	1	0.005	30	0.129
	3	7	0.032	13	0.059	6	0.027
	4	6	0.022	51	0.192	45	0.187
P	5	69	0.363	8	0.043	61	0.289
	6	36	0.192	22	0.107	58	0.279
	7	22	0.097	39	0.208	17	0.081
	8	97	0.538	58	0.368	39	0.223
	9	3	0.022	1	0.011	4	0.032
	10	10	0.075	0	0.005	10	0.075

In order to determine the statistical validity of these differences we have calculated the values of P_z for each difference for every fraction of the results of all four *O*s. These values are given in Table VII. Two columns are given to the results in order for the differences light-heavy, light-intermediate, and heavy-intermediate. In the first column of each pair will be found the values of the differences and in the second column the values of P_z for that difference.

For *A* the differences are small with the exception of 4 values which occur in two series (light-heavy and light-intermediate of Series 2 and 3). These larger values of difference are due to variations of reaction-times of the light judgments. All of the other differences are extremely insignificant and this is especially true, throughout, for the differences between the heavy and the intermediate judgments. The probable errors being large all of the values of P_z are so small as to be entirely without statistical significance.

For *F* all of the differences are considerably larger than those for *A*. The averages for the three differences for the three comparisons are of considerable magnitude. These averages are: light-heavy, 21.5; light-intermediate, 49.8; and heavy-intermediate, 67.2. These results are what one might expect from a consideration of the curves of Fig. 3, which indicates that invariably the reaction-times for all fractions were largest for the intermediate judgments, and in 8 of the 10 fractions the values for heavy judgments were less than those for light. The values for P_z, however, indicate that when the probable errors, which were large for this *O*, are taken into account that the differences are not of statistical significance. The largest value of P_z is 0.542 (heavy-intermediate, Series 9). Of the 30 values of P_z for this *O*, 7 are less than 0.1, 14 less than 0.2, 20 less than 0.3, 25 less than 0.4, and only 5 greater than 0.4.

For *H* the magnitudes of the differences are again relatively large and seem to show relatively great variability for the different comparisons. The averages for this *O* are light-heavy, 16.9; light-intermediate, 69.1; and heavy-intermediate, 54.6. It will be remembered that the curves for *H* (Fig. 4) indicate that the reaction-times for the light and heavy judgments were quite similar while those for the intermediate judgments were invariably longer for all fractions. It will be observed, however, that the differences between the reaction-times of the light-intermediate and heavy-intermediate comparisons are relatively large at first and tend to decrease with progressive practice. In no case, however, are the values of P_z of great statistical

significance. The largest value of P_z is 0.438 (light-intermediate, Series 3). Of the 30 values of P_z, 9 are less than 0.1, 17 less than 0.2, 23 less than 0.3, 26 less than 0.4, and only 4 greater than 0.4.

The differences for *P* are on the whole relatively large but, like those of *A*, show no constant relationship. Indeed the differences for light-intermediate and heavy-intermediate are smaller than those for light-heavy as will be seen from the averages: light-heavy, 34.4; light-intermediate, 25.4; and heavy-intermediate, 27.4. The values of P_z for this *O* are again relatively or completely without statistical significance. The highest value of P_z is the anomalously large one of 0.538. Of the 30 values, however, 15 are smaller than 0.1, 23 smaller than 0.2, 27 smaller than 0.3, and only 3 larger than 0.3.

Thus for all *O*s there seems to be no general tendency of increase or decrease of the size of the values of P_z as the result of progressive practice. For only one *O*, *H*, is there a well-marked tendency for a decrease in the size of the differences with progressive practice. In this case there is also a corresponding decrease in the size of the probable errors so that no marked changes are to be noted in the values of P_z.

We wish to mention that we recognize the fact that for *F* and *H* the values of reaction-time for the intermediate judgments are invariable higher than those for either of the two other categories for all 10 fractions. Thus the probability that this difference is not due to chance is of the magnitude of $1:2^{10}$. In spite of this, when one applies some measure of the significance of the differences such as the values of P_z it would seem that the significance of the bare probability is minimized.

Progressive practice. The effects of progressive practice on the limens and other significant values of the psychometric functions for the relative method have been known for some time. Urban,[16] and Fernberger,[17] have shown that progressive practice tends to increase the values of the coefficients of precision of the psychometric functions of both the greater and less judgments; decrease the size of the interval of uncertainty and also decrease the magnitude of the point of subjective equality. Fernberger has also shown similar effects of

[16]F. M. Urban, Der Einfluss der Übung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271-311.

[17]Fernberger, The effects of practice in its initial stages in lifted weight experiments and its bearing upon anthropometric measurements, this JOURNAL, 27 1916, 261-272.

progressive practice for the absolute method.[18] The present set of results reveals another effect of progressive practice hitherto unrevealed in the experimental literature of the reaction-times for the formation of psychophysical judgments to our knowledge. We find a marked and relatively regular decrease in the reaction-times for all three categories of judgment for the absolute method. As revealed in Table VI and in Figs. 2–5, the decrease is at first relatively rapid and then slower until a level is reached. Hence these curves are of the relatively parabolic form so familiar with one type of the usual learning curve. Wide individual differences are observable, as one would expect, in the size of the average reaction-times at both the beginning and at the end of the practice period. Also individual differences are to be observed in the steepness of the curves. These individual differences in the steepness of the curves can be explained for 2 of our *O*s in a manner which fits entirely with the expected results. The curves for *A* show the least decrease and it will be remembered that this *O* was the most highly practiced in the lifted-weight technique at the beginning of the present experiment. On the other hand the curves for *F* show the greatest amount of decrease as the result of progressive practice and this *O* was one of the two entirely unpracticed *O*s at the beginning of the experiment. The steepness of the curves for *H* (practiced) and for *P* (unpracticed) are both intermediate to those of the other two *O*s. Whether the curve for the unpracticed *P* would have shown a greater amount of decrease if it had not been for his anomalous results in the first two fractions is something about which we have no information. With regard to these early series for *P* it is interesting to note that the values of his intervals of uncertainty (Table I) show a course more or less similar to his reaction-time curves. The values of the intervals of uncertainty start low, increase considerably in magnitude during the first three series and then decrease considerably but not regularly for the rest of the experiment.

Discussion

It had been shown by George that there were considerable differences in reaction-times for judgments of different categories by the relative method in psychophysics. On the basis of these, as at least one criterion, it was argued that the judgments of 'less,' 'equal,' and 'greater' might properly be retained in psychophysical procedures

[18]Fernberger, On absolute and relative judgments in lifted-weight experiments, *ibid.*, 43, 1931, 560-578.

but that other types of 'guess,' 'doubtful,' and 'or' judgments should be eliminated. George also found that the average reaction-times for the three retained categories of judgment varied considerably from one another—that for less being fastest, that for greater in between and that for equal decidedly slower than the other two. It may be pointed out that this is what might have been expected for lifted-weights if the introspective analyses of the formation of judgments in terms of motor set is to be accepted as described by Martin and Müller[19] and by Fernberger.[20] In the presence of such a motor set, occasioned by the lifting of the standard stimulus, the basis for the less judgment in lifted-weights is found to be that the comparison weight goes up easily (seems to fly up) from the table. The basis for the heavy judgment is that the anticipatory motor set is not sufficient to lift the weight readily or that the weight goes slowly from the table and not so high. On the basis of this analysis one would expect the less judgment to have a slightly shorter reaction-time than the greater judgment. Both Martin and Müller and Fernberger found that, although judgments of absolute equality were obtained, the equal judgment was frequently a negative affair of "no difference" rather than of actual equality and further that such judgments very frequently were followed by a subsequent verification process before the judgment was reported. It is obvious then that the equal judgment should have a longer reaction-time than either of the two difference categories inasmuch as it seems to be of a less immediate character. Fernberger has shown that this difference in the nature of the formation of equal judgments and those of difference judgments is due to the fact that the normal instructions seem to set up an attitude in the mind of the *O*, of looking for difference.[21]

Kellogg has gone still further and, on the basis of the criterion of reaction-times, has recommended the acceptance of a two-category plan with the inclusion of judgments of less and greater only rather than a three-category plan which includes the classical three categories of judgment of less, equal, and greater. Kellogg found that the massed reaction-times under the two-category plan were smaller on the average than the massed reaction-times under the three-

[19]L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899, 1-96.

[20]Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 26, 1919, (no. 6), 1-161, esp. 155 f.

[21]Fernberger, The effect of the attitude of the subject upon the measure of sensitivity, this JOURNAL, 25, 1914, 538-543; and Instructions and the psychophysical limen, *ibid.*, 43, 1931, 361-376.

category plan. Unfortunately for our present purpose Kellogg does not give the results of the reaction-times for each of the different categories within the three-category plan.

The present writers are not inclined to accept without question the validity of the criterion of the reaction-time as indicating the validity of one or more categories of judgment and the invalidity of others. In the case of George's results, we believe that there are too few observations for the average to be of any great statistical value. A total of 2529 reaction-times were obtained in all from 5 *O*s and these are distributed among 13 different categories of judgment. A total of 697 cases is the largest for any category of judgment from all 5 *O*s. Furthermore, George publishes no probable errors or any statistical values of the significances of these differences. The present results would indicate that the probable errors of George's averages would have been large and this would tend to decrease the significance of the differences, of course. Similarly Kellogg's results, although more numerous, are treated in such a way as to fail to give the basis of comparison of the reaction-times of the categories of judgment in the three-category plan.[22]

If, however, the reaction-time differences are a valid criterion for the elimination or retention of categories of judgment in psychophysics, the present results would indicate that, for the absolute method, the choice of categories of 'light', 'intermediate,' and 'heavy' are all capable of being formed under a constant attitude and should all be retained. In no case are averages in reaction-times of these three categories significantly different for any *O* for the results of either the entire experiment or for any fraction as indicated by the smallness of the values of P_z. Our differences for the three categories for the absolute method are considerably smaller than those obtained by George for the three major categories of less, equal, and greater for the relative method.

In the case of only one *O* does the rank order of the reaction-times correspond to that of George. For *H* the rank order from the fastest to the slowest average reaction-times is light, heavy, and intermediate. The results for *F* correspond to those of George in the respect that the reaction-times for intermediate are longer than for either of the

[22]We have tried to obtain the original data of the George and Kellogg experiments for purposes of further calculation. Professor K. M. Dallenbach of Cornell University informs us that George's data were destroyed a few years ago. Professor Kellogg is at present away from Indiana University. He writes that he believes that he can find his original data and that he will send them on his arrival at his University.

difference categories but, for this *O*, the times for the heavy judgments tend to be shorter than those for light. In the cases of the other two *O*s, *A* and *P*, there is no conformity with the results of George as the differences are not only very much smaller in the case of our experimental data but the times for the different categories vary one from another without any apparent general tendency.

Conclusions

Obtaining the reaction times of the categories of 'light,' 'intermediate,' and 'heavy' judgments for lifted-weight stimuli for the absolute method in which immediate judgments are obtained for a series of slightly different stimuli but without standard stimuli, we find:

(1) There is a marked and definite shortening of reaction-times as a result of progressive practice. This decrease is relatively great at first and then proceeds more slowly giving a curve of the parabolic type. This reduction in reaction-time is completely similar for all three categories of judgment.

(2) There is a similar decrease in the size of the probable errors of the average reaction-times for all three categories of judgment as the result of progressive practice.

(3) For 2 of the 4 *O*s, although the reaction-times for the 'light' and 'heavy' judgments are very similar, those for the 'intermediate' judgments are invariably slightly longer than for either of the other two categories. For the other 2 *O*s, the average times for all three categories of judgment are invariably similar.

(4) If some criterion of the significance of a difference, such as the values of P_2, is obtained, the differences between the average reaction-times for the different categories of judgment are shown to be statistically without significance. This is true for the averages of all categories for either the entire experimental data as a whole or for any fraction of the experimental data and for every *O*.

(5) If reaction-times may be considered as criteria for the retention or elimination of categories of judgment in psychophysical procedures, the present results clearly and definitely indicate that the categories of 'light,' 'intermediate,' and 'heavy' are properly chosen categories for the absolute method and that they should all be retained in the use of this method.

WHOLE AND PART METHODS WITH VISUAL SPATIAL MATERIAL

By LELAND W. CRAFTS, New York University

The objects of the present experiment are (1) to determine the efficiency of whole and part methods for learning certain samples of visual spatial material, and (2) to secure further data regarding the influence of the material to be learned—its difficulty, unity, coherence—upon the efficiency of the learning.

The first object represents an attempt to investigate the whole-part problem with a type of material very different from any hitherto employed—with material which is not only non-verbal and non-serial, but which also is of the kind sometimes classified as perceptual rather than as motor. Perrin and Klein state that "so far as perceptual learning is concerned the results, in the case of learning to read, are decidedly against the part method" but "to what degree this finding applies to other forms of perceptual learning must be determined separately for each form."[1] One of these 'other forms' is exemplified by the visual diagrams used in the present experiment.

The second object represents a continuation of previous work of the writer.[2] His earlier experiments, together with other literature in the field,[3] suggest that the whole method may be expected to be more advantageous with easy rather than with difficult, and with related rather than with unrelated material. In the present experiment these propositions will be again tested in this new type of learning situation.

METHOD AND PROCEDURE

Subjects. The *S*s were 305 undergraduate men and women. In every one of the five experiments performed three groups of *S*s—each group a class in Psychology—were employed. Since the groups were the same in Experiments I, III, and V, the total number of different groups was nine. No attempt was made to equate the groups.

* Accepted for publication, March 21, 1931.

[1] F. A. C. Perrin and D. B. Klein, *Psychology*, 1926, 280.

[2] L. W. Crafts, Whole and part methods with non-serial reactions, this JOURNAL, 41, 1929, 543-563; Whole and part methods with unrelated reactions, *ibid.*, 42, 1930, 591-601.

[3] L. A. Pechstein, Massed *vs.* distributed effort in learning, *J. Educ. Psychol.*, 12, 1921, 92-97; E. W. Sawdon, Should children learn poems in wholes or in parts? *Forum Educ.*, 5, 1927, 182-197; W. H. Winch, Should poems be learnt by school-children as 'wholes' or in 'parts'? *Brit. J. Psychol.*, 15, 1924, 64-79.

The data to follow will include the records only of *Ss* who completed an experiment, *i.e.* who worked with a given type of material with all of the three learning methods used. The number of such *Ss* was as follows:

Experiment	Group 1	Group 2	Group 3	Total
I	27	33	34	94
II	41	34	39	114
III	36	30	34	100
IV	29	35	27	91
V	32	26	28	86

Materials: (*1*) *Circles.* The diagram shown in Fig. 1 was drawn in black India ink upon a sheet of very heavy white cardboard 21⅝ × 29⅞ in. The 64 small squares measured 2.5 in. each. The circles, always 16 in number, were 1⅜ in. in diam.; they were cut from black cardboard, and were fastened by black-headed clips in the centers of 16 of the squares. They were arranged as irregularly as possible within the limits of the requirement that there should be 4 circles in each quarter of the field. Three of these arrangements, one of which is reproduced in Fig. 1, were used. The location of every one of the 16 circles was different in every arrangement.

(*2*) *Lines.* The diagram, shown in Fig. 2, was laid out upon heavy cardboard as before. The 49 points, 3 in. apart in 7 rows of 7 each, were the heads of brass clips painted black and 5/16 in. in diam. The lines, always 16 in number, were made by connecting pairs of points adjacent vertically, horizontally or diagonally with strips of black paper ¼ in. in width, which were inserted under and held down by the heads of the clips. These lines were arranged as irregularly as possible within the limits of the requirement that there should be 4 lines—one vertical, one horizontal, and one in each of the two possible diagonal directions—in every quarter of the field. Three such arrangements, one of which is reproduced in Fig. 2, were used. The location of every one of the 16 lines was different in every arrangement. The enclosure of two of the points with black circles was intended to facilitate the learning.

(*3*) *Figures.* The same outline of points as that used for the lines was employed, and the points and connecting lines were constructed in the same manner. Every figure was composed of 16 lines, arranged so as to form as irregular a design as possible. One quarter, *i.e.* 4 lines, of a figure was allotted to every quarter of the field. Three of these irregular figures, one of which is reproduced in Fig. 3, were used. They differed greatly in general shape, and the location of every one of the 16 constituent lines was different in every figure.

Instructions. The *Ss* received instruction sheets, of which the following for the lines is a sample. Those for the other materials were similar.

This is an experiment in learning. You will have displayed to you a diagram composed of a large number of black points symmetrically arranged on a white background. Certain pairs of adjacent points are connected by black lines—vertically, horizontally, or diagonally. Your task is to learn the exact location on the diagram of each of these lines. You will have only 120 sec. When the end of that period is announced, turn to the sheet, which you now have, containing an exact replica of the above diagram and reproduce thereon, in its exact location, every one of the black lines.

Presentation. The cardboards carrying the diagrams to be learned were fastened to an easel which was placed on a table in front of the class. The diagram was concealed between exposures by a large sheet of gray cardboard. When exposed it was presented (1) as a whole, (2) by parts, or (3) by combinations of parts. In the pure part method in which a quarter of the diagram was exposed at a time, presentation was always clockwise, *i.e.* upper left quarter (part 1), upper right (part 2), lower right (part 3), lower left (part 4). In the combination part method, used only with the lines and the figures, each 'part' consisted of half the diagram. These halves were presented similarly in a clockwise direction, *i.e.* upper half (quarters 1 and 2), right half (quarters 2 and 3), lower half (quarters 3 and 4), and left half (quarters 4 and 1). In the progressive part method, used only with the circles, the nature and sequence of the parts and their combinations

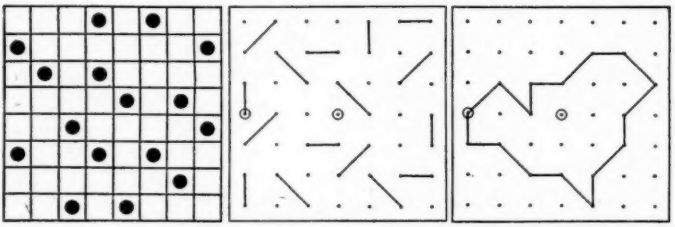

FIG. 1 FIG. 2 FIG. 3

FIGS. 1-3. SHOWING THE KINDS OF MATERIAL USED

are given in the footnote to Table I. This table also shows that in all the part methods—except the progressive part—every quarter or half was presented an equal number of seconds, and that the diagram was exposed as a whole for a brief period at the conclusion.

The duration of presentation, in every experiment except Experiment IV, was such that the average reproduced score would be from 60-70% correct.

The *S*s were told at every trial what method of presentation would be used.

The diagram was exposed as a whole by removing the sheet of gray cardboard that served as the screen; it was exposed by parts—by quarters or halves—by using screens of gray cardboard from which sections had been cut of the desired size.[4]

Rotation of learning methods. In every one of the five experiments the following rotation of the learning methods was employed:

Trial	Group 1	Group 2	Group 3
1	Whole	Pure Part	Comb. Part
2	Pure Part	Comb. Part	Whole
3	Comb. Part	Whole	Pure Part

[4]In Experiment I (circles) *E* placed these screens one at a time over the diagram, removing one and substituting another according to the prearranged plan. In the other four experiments (Experiments II-V) a screen was used that rotated upon a screw and nut device fixed in the center of the diagram and so contrived that it could be instantly removed when the diagram as a whole was to be exposed.

Hence with every kind of material every learning method was employed once in every one of the three possible temporal positions, and once with every one of the three different arrangements of circles and lines or the three different figures used.

Distribution of trials. The distribution of trials for all experiments except Experiment III was as follows: A preliminary trial with complete explanation and illustration. An interval of 5 days. The 3 experimental trials, one with each learning method, at intervals of one week. The distribution for Experiment III was identical with the above except that the 3 experimental trials were given at intervals of 2 days instead of one week.

TABLE I

SHOWING THE EXPOSURE TIMES USED IN EVERY ONE OF THE EXPERIMENTS

Experiment	Material	Method	Time in sec. per part	Time in sec. for whole	Time in sec. total
I	Circles	whole		94	94
		pure part	22	6	94
		prog. part	*	6	94
II and III	Lines	whole		120	120
		pure part	28	8	120
		comb. part	28	8	120
IV	Lines	whole		94	94
		pure part	22	6	94
		comb. part	22	6	94
V	Figures	whole		35	35
		pure part	8	3	35
		comb. part	8	3	35

*In this method, the progressive part, the exposure times and sequence of the parts were as follows: part 1, 12 sec.; part 2, 12 sec.; parts 1 and 2 combined, 16 sec.; part 3, 12 sec.; parts 1, 2, and 3 combined, 24 sec.; part 4, 12 sec.; whole, 6 sec.

Reproduction. Graphical reproduction immediately followed the presentation. *Ss* were allowed as much time as they wished except that after 3 min. had elapsed they were urged to complete their work quickly.

Scoring. Every circle that was in the correct square, and every unrelated line or line that was part of a figure which connected the correct two points, was scored 1. The scoring was always of these constituent elements separately considered, and no part credits of any kind (as for accuracy in general form or pattern alone) were given.

RESULTS

Tables II and III show the whole method to have been reliably superior to either part method for learning the circles and the figures, but not so for the unrelated lines with either long or short interval of

exposure.[5] Neither part method was reliably superior to the other with any kind of material. Table IV shows that with the circles and the figures a majority of the individual *S*s made their highest scores

TABLE II

SHOWING FOR ALL EXPERIMENTS THE AVERAGE NUMBER OF ELEMENTS CORRECTLY REPRODUCED AND THE S.D. OF THOSE AVERAGES

Experiment	Method					
	Whole		Pure Part		Comb. Part	
	Av.	S.D.	Av.	S.D.	Av.	S.D.
I Circles	12.1	3.15	10.2	3.36	10.6	3.01
II Lines	10.1	3.42	10.2	3.66	9.2	3.52
III Lines, short interval	11.6	3.14	11.7	3.62	10.6	3.44
IV Lines, short exposure	7.1	3.05	6.1	2.40	6.6	3.40
V Figures	12.1	3.79	10.2	4.12	9.8	4.20

TABLE III

SHOWING THE DIFFERENCES BETWEEN THE AVERAGES GIVEN IN TABLE II AND THE S.D. OF THOSE DIFFERENCES

Experiment	Comparison*	Diff.	$S.D._{Diff.}$
I Circles	Whole–Pure Part	1.9	0.474
	Whole–Comb. Part	1.5	0.445
	Pure Part–Comb. Part	−0.4	0.468
II Lines	Whole–Pure Part	−0.1	0.469
	Whole–Comb. Part	0.9	0.460
	Pure Part–Comb. Part	1.0	0.476
III Lines, short interval	Whole–Pure Part	−0.1	0.479
	Whole–Comb. Part	1.0	0.466
	Pure Part–Comb. Part	1.1	0.499
IV Lines, short exposure	Whole–Pure Part	1.0	0.479
	Whole–Comb. Part	0.5	0.576
	Pure Part–Comb. Part	−0.5	0.438
V Figures	Whole–Pure Part	1.9	0.601
	Whole–Comb. Part	2.3	0.609
	Pure Part–Comb. Part	0.4	0.629

*In every comparison the score of the second method named has been subtracted from that of the first. Hence a minus sign before a difference shows that the score for the second method was the greater.

[5]This absence of any reliable difference between the three learning methods with the lines was the reason for performing three experiments with this particular material. It was thought possible that through variation in time of exposure or in length of interval some condition might be discovered under which one of the methods would become reliably superior to the others. But, as the data show, although decreasing the exposure time seems to have increased somewhat the effectiveness of the whole method, the attempt was unsuccessful.

with the whole method, whereas with the lines no method was superior or inferior for more than 50% of them.

Table V shows the influence of practice effect to have been definitely less with the circles and the figures than with the unrelated lines. It was therefore with the material where learning method seems to have been of least significance that the effect of practice alone, of positive transfer from one trial or diagram to another, most clearly revealed itself.

TABLE IV

SHOWING FOR ALL EXPERIMENTS THE PERCENTAGE OF *Ss* FOR WHOM EVERY LEARNING METHOD WAS SUPERIOR OR INFERIOR

Experiment	Method					
	Whole		Pure Part		Comb. Part	
	Superior	Inferior	Superior	Inferior	Superior	Inferior
I Circles	53.8	21.0	23.6	46.2	22.6	32.8
II Lines	41.2	25.0	37.3	30.3	21.5	44.7
III Lines, short interval	42.9	24.5	42.4	26.5	14.7	49.0
IV Lines, short exposure	37.7	21.9	26.7	37.1	35.6	41.0
V Figures	56.1	15.0	18.3	42.5	25.6	42.5

TABLE V

SHOWING THE INFLUENCE OF PRACTICE EFFECT BY GIVING THE AVERAGE SCORES FOR ALL GROUPS ON THE THREE EXPERIMENTAL TRIALS, LEARNING METHODS BEING DISREGARDED

Experiment	Trial 1	Trial 2	Trial 3
I Circles	10.8	10.7	11.4
II Lines	8.7	10.1	10.7
III Lines, short interval	10.1	11.5	11.6
IV Lines, short exposure	5.5	6.7	7.3
V Figures	10.7	11.0	10.4

DISCUSSION

(*1*) *The whole method.* As the data in Tables II, III, and IV show, the whole method was more effective with the circles and the figures than it was with the unrelated lines. Explanation of these differences in its value requires, we believe, consideration of the two factors of difficulty and relationships.

As to difficulty, an index thereof may be obtained by dividing the average reproduction score by the total exposure time, which will give the average score obtained per second of learning time. The results of such division are: Figures, 0.35; Circles, 0.13; Lines, 0.08; Lines

(short interval), 0.09; Lines (short exposure), 0.08. The figures show that the whole method was more advantageous with the easier materials and much less effective with the more difficult. Since the learning in the present experiment was unspaced, such results are in principle harmonious with Pechstein's view that with a 'hard' problem the whole method with massed practice is apt to be at a particular disadvantage.[6]

The nature of these differences in difficulty was in part quantitative, even though the number of elements to be reproduced was with all materials the same, namely 16. For, as reference to the figures given earlier will show, while correct reproduction of a circle required the *S* to recall the location of only *one* square, reproduction of an unrelated line required him to recall the location of *two* separate points. With the figures it is of course evident that a given line could be recalled as an isolated line, or as a line joining another already reproduced, or as one connecting two lines previously localized, with corresponding differences in this purely quantitative aspect of difficulty.

But it would, we think, be generally agreed that these differences in the difficulty of our materials are not reducible to simple quantitative factors alone, but that they are due also to the more qualitative factor of relationships. It seems certain that the lines were hardest, not merely because their reproduction required localization of more elements, but also because they were the most difficult to relate, to organize into coherent patterns. Similarly the figures were easiest for one reason because such organization of elements into patterns was given in the material itself. In consequence we consider that our data distinctly favor the conclusion that, as is commonly supposed, the whole method may be expected to be more advantageous with the more related as well as with the easier material.

As to why the whole method was not decidedly more efficient with the figures than with the circles, we have no conclusive answer. It may be that, for reasons already given, the circles were quantitatively easier. It may also be that—as the unsolicited comments of certain *S*s suggest—they were unusually easy to organize and relate, and this in spite of our attempt to arrange them on the diagram in as irregular and unrelated a manner as possible. With reference to this second possibility it is worth pointing out that with material which is easy to organize, the relationships which *S* establishes for himself

[6] Pechstein, *op. cit.*, 45.

might produce patterns which would facilitate learning just as much as would patterns given objectively in the material itself. In the former case *S* creates his own forms, relates elements in ways most natural for him; but in the latter case he must for the most part accept the material as it is given to him and adjust himself to forms devised by the experimenter, regardless of the ease or difficulty which they chance to possess for himself.

(*2*) *The part methods.* As to why the two part methods used should have failed to yield reliably different results, we have no entirely adequate explanation. Theoretically one might suppose that since part methods of either the progressive or the combination type are intermediate in form between the whole and the pure part method, they should yield scores which would likewise be intermediate. In the one case, with the circles, in which we used a true progressive method it did occupy such a position (see Table II). But the combination method was thus intermediate in score only with the lines, short exposure. With the figures, and in both experiments with the lines, long exposure, it gave the lowest scores of all. Such results can scarcely be said to agree with those of Pechstein for mazes[7] and nonsense syllables,[8] of Reed for poetry and prose,[9] of Gopalaswami for mirror-drawing,[10] or of Crafts for card sorting[11] and for substitution tests with massed practice.[12] For in all these cases a progressive type of part method was found to be superior to the whole method, to the pure part method, or to both.[13] This divergence between our present results and those of previous experiments may be due to peculiarities of our visual-spatial material, which was very different from that used in any earlier study. It may also be due to the nature of the combination method which we used with all materials except the circles, and which was a kind of part method somewhat different from any hitherto employed. In this method, it will be recalled, the parts were halves composed of quarters successively recombined with

[7]Pechstein, Whole *vs.* part methods in motor learning: a comparative study, *Psychol. Monog.*, 23, 1917, (no. 99), 68, 76.

[8]Pechstein, Whole versus part methods in learning nonsensical syllables, *J. Educ. Psychol.*, 9, 1918, 381-387.

[9]H. B. Reed, Part and whole methods of learning, *ibid.*, 15, 1924, 107-115.

[10]M. Gopalaswami, Economy in motor learning, *Brit. J. Psychol.*, 15, 1925, 226-236.

[11]Crafts, Whole and part methods with non-serial reactions, *loc. cit.*

[12]Crafts, Whole and part methods with unrelated reactions, *loc. cit.*

[13]It might be noted, however, that J. W. Barton (Smaller *vs.* larger units in learning the maze, *J. Exper. Psychol.*, 4, 1921, 418-430) found a progressive inferior to a pure part method for learning mazes.

each other. It may also be, as certain *S*s remarked, that this presentation of a quarter of the diagram in two different combinations, *e.g.* of the third quarter first with the second and then with the fourth, was 'confusing' in that it tended to force the elements of a given quarter into two different, and often interfering, sets of relationships. If such an effect was produced, the failure of the combination method to yield higher scores might of course be thereby explained.

Summary

The aims of the present experiment were (1) to determine the relative efficiency of whole and part methods for learning certain samples of visual spatial material, a kind not used in any previous investigations of the whole-part problem, and (2) to secure further data as to the influence upon their efficiency of differences in the difficulty of the materials learned and in the degree of unity and coherence which they possess. Three types of visual material—irregular arrangements of circles and of unrelated lines and irregular geometric figures—were employed. Every type was presented to three groups of *S*s by three methods, the whole, the pure part, and a combination or a progressive part. The total exposure times were brief, ranging from 35 to 120 sec.; partial learning only was secured with most of the *S*s; reproduction was immediate; scoring was of constituent elements only. The data show the whole method to have been reliably superior with the circles and the figures only; with the much more difficult unrelated lines, even with variations in the length of exposure and of interval, no method was reliably superior to any other. These results in general agree with theoretical expectation and with the results of experiments with other materials. They suggest (a) that in the field of visual 'perception' as well as in that of motor or of verbal learning, neither the whole nor any one form of a part method will invariably be superior; and (b) that the whole method can be expected to be especially advantageous with easier and with more closely related materials.

THE RELATION BETWEEN MUSCULAR TENSION AND QUALITY OF PERFORMANCE

By Elizabeth Duffy, Sarah Lawrence College

The relation between quality of performance and tension of the skeletal muscles has been the subject of a number of recent investigations. A review of these studies will not be attempted here; it will suffice to point out that the reports have included both the statement that increases in tension of the muscles resulted in increased efficiency of performance,[1] and the statement that the poorest performers in a given task were the *S*s with greatest tension.[2] These statements are not necessarily incompatible, but they suggest the need for further analysis of the problem. Such an analysis is attempted in the present paper, which is based upon a series of experiments reported in part in an earlier publication.[3]

Experimental Procedure

The *S*s, it will be recalled, were 18 children varying in age from 2 yr., 11 mo. to 3 yr., 10 mo. (median, 3 yr., 3 mo.).[4] Graphic records were obtained of the pressure upon dynamographs held in each hand during a tapping performance and during discriminative reactions to 3 kinds of pictures (an automobile, a horse, and a street car), to only one of which the *S*s were to respond by closing a reaction-key. Forty pictures were presented in random order at precisely 4-sec. intervals. This experiment was repeated 5 times with every *S*, within a period of 4 months.

Discriminative reactions were scored 'correct' when the reaction-key was closed at any time during the 4-sec. interval after the appearance of the picture

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The study was made possible through a fellowship from the Committee on Child Development of the National Research Council, and through the generous aid of Professor R. S. Woodworth, and of the Child Development Institute of Teachers College, Columbia University.

[1]A. G. Bills, The influence of muscular tension on the efficiency of mental work, this Journal, 38, 1927, 227-251.

[2]Buford Johnson, Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 11, 1928, 329-341.

[3]Elizabeth Duffy, The measurement of muscular tension as a technique for the study of emotional tendencies, this Journal, 44, 1932, 146-162.

[4]Tension scores for the discriminative reactions are based on the records of 16 *S*s, since two of the *S*s (*P* and *Q*) failed to comply with the instructions. Performance scores are based on the records of 15 *S*s because a third *S* (*R*), who disliked the sound of the buzzer, frequently pressed the reaction-key too lightly to close it and thus left an inaccurate graphic record of his performance. Tension scores for the tapping test are based on the records of 17 *S*s, since one *S* (*Z*) could not be obtained for this part of the experiment. Performance scores are based on the records of 16 *S*s, because of an error in tabulating the score of a second *S* (*G*).

of an automobile or was not closed after the appearance of the picture of a horse or a street car. They were scored 'false' when the reaction-key was closed after the appearance of a horse or a street car, and 'omitted' when it was not closed after the appearance of an automobile.

Type of tension tracing (with respect to smoothness) was determined from a classification of the tension tracings from the used hand during the tapping test, since it was here that individual differences appeared in most exaggerated form. An individual who had had no part in the experiment and was not familiar with the results was asked to group the tracings into three categories on the basis of degree of *smoothness* or *regularity* and irrespective of the height of the tracing (or *degree* of tension). The first category comprised tracings which were very irregular, the second, those which were moderately irregular, and the third, those which were very smooth. Division into categories was somewhat arbitrary, since the tracings are not sharply divisible into types. A second classification made by the same individual three months later resulted in two changes in the grouping. A tracing previously classed as Type 1 was called Type 2, and one formerly called Type 2 was classed as Type 3. Because of the uncertainty of opinion about these two tracings they were both put under Type 2, the most indeterminate category. The tracings, with their classification, may be seen in the accompanying plate.

Results

(*1*) *Degree of muscular tension and quality of performance.* The relation between quality of performance and average degree of muscular tension (as measured by pressure upon the dynamographs) is moderately close—high tension tends to accompany poor performance. The coefficient of correlation between tension and performance score is, for the discriminative reactions, -0.47, and, for the tapping, -0.48 (Table I). These coefficients of correlation were secured by the method of rank-differences. Since there were a number of shared ranks for the discriminative reactions, the relation between this performance and muscular tension was obtained also in terms of the Pearson r. The coefficient secured by this method was -0.54.

The correlation between chronological age (C.A.) and percentage of correct discriminative reactions ($r = 0.48$) suggests that age may have exercised a determining influence upon the quality of this performance. Age appears, however, to be unrelated to the number of taps ($r = 0.10$). Further analysis shows that age cannot account for the relation between tension and percentage of correct discriminative reactions, since C.A. and tension show no correspondence ($r = -0.10$).

The mental age (M.A.) and the intelligence quotient (I.Q.), as obtained from the Kuhlmann-Binet test, yield rather negligible coefficients of correlation with quality of performance and with average tension (Table I). Summarizing the data, we may say that in this

TYPE 1

TYPE 2

TYPE 3

FIG. 1. TENSION TRACINGS FROM THE USED HAND DURING THE TAPPING PERFORMANCE

The first half of the tracing represents the right hand; the second half, the left hand. The letters on the left refer to the *S*s.

experiment degree of muscular tension is moderately closely related to quality of performance in both discriminative reactions and tapping (high tension scores tending to accompany poor performance), and that it is negligibly related to C.A., M.A., and I.Q.

TABLE I

COEFFICIENTS OF CORRELATION BETWEEN TENSION AND PERFORMANCE
(Values in parentheses obtained by the product-moment method, the others by the method of rank-differences)

		Percentage of correct discriminative reactions	Number of taps	Av. tension in both hands (discriminative reactions)	Type of error (discriminative reactions)	C.A.
Av. tension in both hands	r	−.47 (−.54)	−.48		(.14)	
	P.E.	.14 (.12)	.14		(.17)	
Regularity tension tracing (tapping)	r	(.60)	(.23)	(−.31)	(.59)	(.43)
	P.E.	(.12)	(.16)	(.16)	(.12)	(.13)
C.A.	r	.48 (.51)	.10 (.05)	−.10 (−.12)		
	P.E.	.14 (.12)	.18 (.17)	.18 (.17)		
M.A.	r	.11	.26	−.13		
	P.E.	.18	.16	.17		
I.Q.	r	−.12	.35	−.23		
	P.E.	.18	.16	.17		

(*2*) *Type of tension tracing and quality of performance.* The percentage of correct discriminative reactions is related not only to the *degree* of muscular tension but also to the *type* of tension tracing with respect to smoothness or regularity. Very irregular tracings are associated with poor performance, while smooth tension tracings tend to be associated with good performance. The coefficient of correlation between quality of performance and type of tension tracing is 0.60, which suggests that *fluctuations* in tension are at least as closely related as *degree* of tension to quality of performance in such a task as the making of discriminative reactions. The close relationship between these two factors (*i.e.* fluctuations in tension and percentage of correct discriminative reactions) is the more remarkable in view of the fact that tension fluctuations were judged from the tapping records and not from the records of discriminative responses.

Regularity of tension tracing and number of taps do not appear to be related, since the coefficient of correlation between the two factors is only 0.23.

The significance of irregularities in the tension tracing is still somewhat open to question, but it appears probable that this aspect of the tracing is indicative of degree of coördination, which, in turn, is dependent upon the ability to inhibit reactions. If this assumption is tenable, the fact that smoothness of tracing appears to be rather closely related to percentage of correct discriminative reactions and very negligibly related to number of taps may be readily explained by the relatively greater demands of the former task upon the ability to inhibit and effectively coördinate responses. Coördinative ability undoubtedly increases as the child advances in age, and we find, as we should expect, a correlation of 0.43 between C.A. and smoothness of tension tracing. But this coefficient of correlation makes it possible to interpret our data in either of two ways. It suggests, on the one hand, that percentage of correct discriminative reactions is correlated with C.A. and with smoothness of tension tracing because, as the child grows older, he increases in ability to inhibit and coördinate his responses, and growth in this ability is the source of improvement in discriminative reactions as measured under the conditions of this experiment. But it suggests, on the other hand, that the correlation between smoothness of tension tracing and percentage of correct discriminative reactions may be the accidental result of the correlation of each of these factors with C.A. The latter conclusion seems less tenable than the former, both on general theoretical grounds and because the relationship between smoothness of tracing and percentage of correct reactions is somewhat closer than that between C.A. and either of these factors. But conclusions must be drawn with caution, since rather wide differences in coefficients of correlation are not necessarily significant when the number of cases is small.

(*3*) *Tension tracings and discriminative reactions of individual subjects.* The relation between quality of performance and degree and fluctuations of tension can be usefully investigated through an analysis of the records of individual *S*s. A study of the best performers and the poorest performers may yield information upon the factors conducive or detrimental to success. Our analysis will be confined to the discriminative reactions, since we have five records of this performance and only one record of tapping. The data referred to in this section will be found in Table II.

The percentages in the discriminative reactions are based, for all except 3 *S*s, on 5 records of 40 responses, or 200 responses in all. In

the cases of *A*, *H*, and *G* special circumstances made it advisable to base the results on 4 records. The percentages in the table are given to the nearest whole value.

TABLE II

TENSION AND PERFORMANCE SCORES, TYPE OF TENSION TRACING, AND AGE

S	Av. tension (in mm.)		Discriminative reactions (in %)			No. taps‡	Type of tension tracing	Age (in mo.)
	Tapping	Discrim. reactions	Correct	False	Omitted			
A	22.7	18.3	99	1	1	330	2	37
B	31.2	22.5	98	1	1	253	2	46
C	25.7	15.2	98	2	1	291	3	45
D	17.0	17.7	97	2	2	295	3	35
Y	29.9	20.7	96	2	3	335	2	41
F	18.4	20.1	96	1	3	262	3	39
G	28.3	18.7	96	3	2	§	2	41
H	15.0	17.7	95	1	4	320	3	43
I	33.2	20.6	94	2	4	293	2	40
J	33.5	17.8	91	6	4	318	1	39
K	26.4	16.4	91	7	2	292	2	39
L	37.0	24.7	89	8	4	269	1	40
M	52.0	22.1	89	5	6	250	1	37
N	27.0	21.1	87	10	3	303	2	35
Z†	—	24.0	82	16	2	—	—	35
P†	38.5	23.4*	—	—	—	261	1	37
Q†	48.6	23.1*	—	—	—	217	1	35
R†	19.9	21.7	—	—	—	259	3	40

*These scores are not strictly comparable with the others in the column. They are the scores of the 2 *Ss* (mentioned in footnote 4) who failed to comply with the instructions for the discriminative reactions.

†See footnote 4 for an explanation of the omissions in the data of these *Ss*.

‡Not comparable with norms for tapping because of the heavy stylus used in this experiment.

§Omitted because of error in tabulation.

Best performers. The best performer in the discriminative reactions (and second best performer in tapping) is *A*, who has 99% of correct discriminative responses. His rank in tension is tenth, and his tension tracing is of Type 2, that is, moderately irregular. He holds rank 11.5 in age, there being only four *Ss* who are as young or younger than he. *B* and *C*, who have 98% of correct responses, are our next best performers. *B* ranks third in tension, has a moderately irregular tension tracing, and is our oldest *S*. *C* has the lowest tension score and has an exceptionally smooth tension tracing. He ranks second in age. Our fourth best performer, *D*, has twelfth rank in tension and shares with two other *Ss* the lowest rank in age. His tracing is exceptionally smooth.

Of these four good performers, one has a high tension score, one a very low tension score, and the other two have tension scores which are distinctly below the median but are not the lowest scores recorded. It is significant that no one

of these *Ss* has a tension tracing of Type 1—that is, exceptionally irregular. Two have moderately irregular and two very smooth tracings. Good performance tends to be accompanied by tension scores below the median and by tension tracings which are relatively smooth. *A* and *D*, who excelled in spite of being among the youngest *Ss*, have low, but not the lowest, tension scores and tension tracings of Types 2 and 3 respectively.

Before considering the records of the poorest performers, it is of interest to examine the records of the two *Ss* (*P* and *Q*) who failed to comply with the instructions for the discriminative reactions.[5] *Q* seemed unable to comprehend the requirements of the situation (his M.A. was the lowest in the group), and *P* failed apparently because of emotional stress. *P* was an exceptionally excitable child, frequently involved in "scenes" at home. In the nursery school great difficulty was experienced in persuading her to go for her physical examination or for her mental test. The nursery school records state that *P*'s "chief difficulty is her inability to adapt herself easily to new situations and people. She seems terrified and cries very hard." The tension scores of *P* and *Q* are among the highest recorded, in spite of the fact that *P* seldom closed the reaction-key—a circumstance which would tend to lower her tension scores. Their tension tracings are of Type 1, very irregular. Inability to meet the requirements of the situation was associated with high and exceptionally irregular tension.

Poorest performers. Among the 15 *Ss* for whom scores for the discriminative reactions could be obtained, *Z* was the poorest performer, with 82% of correct responses. His rank in tension is second. Since he did not take part in the tapping test, his type of tension tracing is not recorded. Inspection of his other records indicates, however, that his tracings are undoubtedly of Type 1—that is, markedly irregular. He is one of the three youngest *Ss*.

N is second in inaccuracy of performance, having 87% of correct responses. He holds fifth rank in tension and has a moderately irregular tension tracing. He is one of the three youngest *Ss*.

L and *N*, each with 89% of correct responses, rank next among the poorest performers. *L* has the first rank in tension and has a tension tracing which is very irregular (Type 1). She holds rank 7 in age. *M* holds the fourth rank in tension and has an exceptionally irregular tension tracing (Type 1). His rank in age is 11.5.

These poor performers have, in every instance, high tension scores and, in three instances out of four, exceptionally irregular tension tracings (Type 1). It will be recalled that the two *Ss* who failed to comply with the instructions for the performance had also high tension scores and exceptionally irregular tension tracings. Apparently, with these *Ss* and for this type of performance, high and irregular tension tracings are either distinctly unfavorable for efficiency of performance or are the result of awareness of difficulty or failure. Only one *S* with a tension rank in the upper third gave a performance which ranked above the lowest third, and of our four poorest performers no one has a rank in tension lower than rank 5. Not a single *S* with a tension tracing of Type 1 (exceptionally irregular) gave a performance as good as the median, and all but one of our poorest performers have tension tracings of this type.

[5] See footnote 4.

Summarizing these data, we conclude that poor performance was accompanied by high tension scores and very irregular tension tracings. Good performance was accompanied by tension scores of various magnitudes from low to high, but moderate or low scores occurred most frequently. The tension tracings of good performers were never of the very uneven type.

(4) *Type of tension tracing and type of error.* The smoothness of the tension tracing of an *S* is related not only to his percentage of errors in discriminative reactions but also to his predominant *type* of error in this performance.

It will be recalled that errors were divided into two categories: (1) *false* responses (closing the reaction-key when no response was demanded), and (2) *omitted* responses (failing to close the reaction-key when a response was necessary).

Disregarding the total percentage of errors and considering merely the relation between percentage of false responses and percentage of omitted responses, we can divide our *S*s into three categories: (a) those whose errors are predominantly false responses; (b) those whose errors are approximately equally divided between false and omitted responses, *i.e.* a difference of less than 2% of all responses; and (c) those whose errors are predominantly omitted responses. Relating these data to smoothness of tension tracing, we obtain as the coefficient of correlation 0.59 (Table I). There is a tendency for errors which are predominantly *false* responses to be associated with unevenness of tension tracing. Conversely, errors which are predominantly *omitted* responses tend to be associated with a smoother tension tracing. No *S* with a tension tracing of Type 1 had, among his errors, predominantly omitted responses, and no *S* with a tension tracing of Type 3 had, among his errors, predominantly false responses. Since false responses represent an excess of reaction, a deficiency of inhibition, we have here support for our assumption that degree of smoothness of the tension tracing is indicative of the *S*'s tendencies with respect to inhibition, a factor which undoubtedly plays a major rôle in that complex process which we refer to as coördination.

Summary

The quality of performance of our *S*s in discriminative reactions and in tapping was shown to correlate moderately closely with the degree of muscular tension as measured by pressure upon the dynamographs. Poor performance was almost invariably accompanied by

high tension, while good performance was accompanied by tension scores of various magnitudes from low to high, but moderate or low scores occurred most frequently.

The quality of performance in discriminative reactions, but not in tapping, correlated rather closely with the smoothness of the tension tracing from the used hand during the tapping performance. Smoothness of the tracing correlated also with C.A. and with type of error in the discriminative reactions, *Ss* whose tension tracings were most irregular tending to be among the younger *Ss* and to make errors which were predominantly *false* as opposed to *omitted* responses. It seems probable that excessive fluctuations in tension represent poor inhibition; that lack of inhibition is a severe handicap in certain types of performance; and that degree of inhibition increases with age.

Discussion and Conclusions

The results obtained in this experiment, as given here and in the earlier report,[6] suggest the following hypotheses in regard to muscular tension and its relation to performance.

Increases in tension of the skeletal muscles appear to represent increases in excitation. They may result from any type of stimulation, but are found in most extreme degree under the influence of what are commonly termed exciting 'emotional' stimuli. They seem to be particularly characteristic of states of 'effort,' and their association with the excited (or sthenic) emotions is probably due to the fact that such emotional states are notably 'striving' or 'effortful.'

If the foregoing hypotheses are correct, we should expect to find increased tension of the skeletal muscles when the individual is confronted with any problem of adjustment, provided only that he attempts to meet the problem and does not 'give up.' The more strongly motivated the individual or the more difficult the situation which confronts him, the greater would be his muscular tension, or his effort to adjust. For muscular tension is conceived to be a state of alertness or preparedness, which makes it possible to act with greater speed and force.

Granting, then, that the behavior of the muscularly tense individual will give evidence of greater energy output, will have a more 'dynamic' quality, we need to inquire as to the effects of these characteristics upon the quality of performance of a given task. Other factors being equal, we should expect a greater output of work

[6] Duffy, *op. cit.*, 148-162.

per unit of time. We should expect also to find evidences of the expenditure of greater energy or force in the performance of the task. Some of this energy would doubtlessly be overflow energy, adding in no obvious way to the efficiency of performance. It may be, however, that even such apparently useless reactions as restless movements, pounding, and speaking with great intensity, serve the function of facilitating other more useful reactions. At any rate, during states of great excitation (*i.e.* great muscular tension), we should expect a greater output of work and a greater expenditure of force per unit of time. A common observation of such a state is found in the expression "working fast and furiously."

But more important than the *quantity* of the work produced under such circumstances is the question of the *quality* of performance to be expected under these conditions. Is the quantity output at the expense of quality, or does the excess energy available result in improvement in quality as well? The answer to this question may well vary with different performances, with different individuals, and even with the same individual under different circumstances. In all except the simplest of performances (as, for example, a tapping performance of the type used in this experiment) quality of work must depend not alone upon the degree of excitation, but also, and more fundamentally, upon the quality of coördination, or the proper directing of the available energy. It seems probable, though not clearly demonstrated, that coördination of response becomes more difficult under high degrees of excitation, because under these conditions impulsion is probably greater, and it is a commonly acknowledged fact that coördination necessarily involves inhibition. It is not surprising, therefore, that many individuals, under the influence of excitative stimuli, make uncoördinated and ineffective responses. But this is not the inevitable result of great excitation, for there are other individuals who have developed, in varying degree, the ability to coördinate effectively and thus produce good work under these circumstances. Indeed, it seems probable that individuals who can successfully meet the hazard to coördination which is imposed by great excitation will find the quality of their work greatly improved by the dynamic and facilitating effects of such states. It is not, perhaps, an unusual circumstance for such individuals to produce under great excitation work of a quality superior to anything they could produce under ordinary circumstances. This may afford an explanation of such an experience as writing under stress a paper

which, when read later, seems to have been written by some one else because the acuteness of analysis and the facility of expression seem foreign to the writer.

Though some individuals may find high tension conducive to good performance, all must necessarily find states of tension very wearing. As a consequence, prolonged periods of work at high tension would be well-nigh impossible, and individuals who habitually work under great tension should probably arrange for shorter work periods and more frequent periods of rest. Under these circumstances their total output of work might nevertheless remain equal to that of an individual working under low tension over longer periods of time.

In our experimental results high degrees of muscular tension were usually found associated with a performance of poor quality. It may well be that these results represent the typical situation, individuals who can perform excellently under very high tension being the exception. It is possible, however, that high tension is a greater liability for young children than for adults, since the latter are likely to have developed more highly their ability to inhibit and to coördinate reactions. In fact, even with our group of *S*s, where the age range was small, there was a tendency for increase in age to be accompanied by increase in smoothness of the tension tracing, a phenomenon which we considered indicative of increased inhibition and, perhaps, increased coördination.

It is not to be assumed that high tension is equally advantageous or disadvantageous for every type of activity. Muscular activities may be handicapped by high tension, since it diminishes ease of movement. Other activities requiring speed and alertness, but not emphasizing choice with its accompanying requirement of inhibition, may in general be favored by relatively high degrees of excitation. Sudden insights and the facile flow of words may, for example, be encouraged, while the careful weighing of evidence may be made more difficult. High degrees of excitation, and consequently of muscular tension, may increase the efficiency of performance of a definite task for which the individual is 'set,' but may decrease flexibility of reaction and thus make more difficult the adjustment to new and unexpected situations.

It appears probable, on the basis of the foregoing discussion, that a moderate degree of tension offers the greatest advantages, since very high tension probably tends to be disruptive and very low tension probably involves lack of alertness or effort. The handicapping

effects of very high tension are clearly shown in our results. Those of low tension are more problematical and have not been demonstrated in this experiment. Certain other investigators have reported, however, instances of increased efficiency under increased tension. A. G. Bills,[7] for example, has shown that a group of individuals performed certain tasks more efficiently under a voluntarily assumed and individually different 'optimum' tension. Whether voluntary and involuntary tensions of the muscles are comparable in their effects may be debatable, but it seems reasonable to suppose that tension could be too low as well as too high for efficient performance. Perhaps diurnal fluctuations in the curve of work could be explained on the basis of tension changes and their relation to efficiency. Tension may in general be too low immediately after waking and too high later in the day, and individual differences in tension variations may account for the observed differences in the work curve of different individuals.

Subsequent investigations in this field should consider, among others, the following problems: (1) To what extent does the relation between muscular tension and quality of performance vary with (a) different tasks, (b) different individuals, and (c) adults and children? (2) Are voluntary and involuntary tensions comparable in their effects? (3) What is the relation between various degrees of excitation, or of muscular tension, and the ability to inhibit and coordinate reactions? Answers to these questions should give us important information about optimum work conditions for different tasks and for different individuals.

[7] Bills, *op. cit.*, 229-250.

PRACTICE AND THE WORK DECREMENT

By Edward S. Robinson and Florence R. Robinson

In a recent paper Glaze shows that where a highly monotonous task is performed every day or on alternate days for a considerable period, the decrement within the day's work grows progressively less marked.[1] Practice thus seems to develop a resistance to those losses in efficiency which characterize continuous work. The task studied by Glaze consisted in the writing of *ababab*, etc., as rapidly as possible for a period of 20 min. One *S* did this work every day for 36 days, another every day for 32 days, and a third on alternate days for 23 days. The average work decrement in terms of speed of writing is much more pronounced during the first 5 days than it is later. This is true for all of his *S*s.

While common observation would indicate that it is possible to build up through practice a resistance to work decrement, the experimental study of this process is interesting and important. In the first place it is desirable to test the conclusion of casual experience. In the second place, there is a certain sense in which this increasing resistance to decrement runs counter to logical expectation. Practice typically brings about an increased average speed of performance and one might expect, therefore, that the practiced performance, since it is carried on at higher speed, would be more, rather than less, likely to suffer decrement. It is possible that experiment may reconcile the generally accepted fact of decreasing decrement with this contrary but not unreasonable expectation. There is, in short, the possibility that experimental efforts to demonstrate the influence of practice on the decrement may give clues as to the more precise mechanism of that influence.

Certain of the experimental results that we shall present in this paper are similar to those of Glaze and to a considerable extent simply corroborate his findings. We have also considered changes in the amplitude of the writing of our *S*s and the data thus secured introduce additional facts that need consideration in connection with this problem.

Subjects. Fifteen men, all Yale undergraduates, served as *S*s in our experiment. These men were recruited from the employment office and worked for pay. The records of only 10 of the men were utilized; the other 5 were dropped because of absence from one or more of the experimental sittings.

Procedure. The *S*s worked as a group. The sittings began at 8:30 a. m. on 5 days a week and continued for 3 weeks, *i.e.* for 15 days. The task consisted of writing *ababab*, etc., as rapidly as possible for a 20-min. period. The writing was done in a small, bound composition book. The page was turned at the end of each minute, the time for the turn being called out by the experimenter. Each *S* was provided at the beginning of a sitting with three sharpened pencils.

*Accepted for publication March 6, 1931. From the Psychological Laboratory of Yale University.

[1] J. A. Glaze, The effects of practice on fatigue, this Journal, 42, 1930, 628-630.

The *Ss* were directed to work for speed. Nothing was said about the maintenance of a given quality of handwriting except that the *Ss* were told to keep writing on the ruled lines of the composition book. After certain general explanations had been given, the special experimental 'set' was established by the following verbatim instructions which were spoken by the experimenter:

You are to write the letters, *ababab*, as fast as you can until I tell you to stop. Keep your writing on the line. Use your natural handwriting and write the full length of the line at each end. When I say 'Turn', turn over the page thus and keep going. If you make a mistake, don't stop to correct it, but keep going. Do your best all of the time. Is this clear? I will tell you when to stop. Are there any questions? When I say 'Go!' open your notebook thus and start writing. Are you ready? Go!

At the end of the sitting the *Ss* were asked not to practice the writing or discuss the experiment between sittings. After the first day the instructions were simplified by deleting all of the above paragraph between the command: "Do your best all the time" and "Are you ready?"

Of the men whose records are included in our results, all seemed to be putting forth something like a maximum effort throughout the experiment.

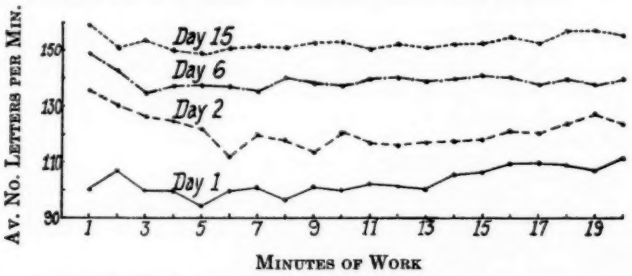

Fig. 1. Work Curves for Days 1, 2, 6, and 15, in Terms of Speed of Writing (Average performances of 10 *Ss*.)

Results

In Fig. 1 we present composite work curves for the group of 10 *Ss*. In the interests of simplification we have given the curves for Days 1, 2, 6, and 15 rather than for the entire series. The curve for Day 1 shows a possible slight early decrement. A more definite tendency, however, is a gradual and continuous rise in letters written per minute. This does not mean that the factors normally producing decrement were absent, but rather that even within the single experimental sitting the factors producing improvement were very strong. Evidence for such an interpretation is presented by the great increase in general speed from the first to the second day.

The curve for Day 2 shows the characteristic work decrement. The improving influences of practice had by this time diminished as shown by the smaller day to day improvement following Day 2. Such decrement as appeared had spent itself by the middle of the 20-min. period, however, and from that point on there is evidence of further practice improvement.

The curve for Day 6 again shows a decrement though it is less in magnitude and duration than that for Day 2. Improvement following the decrement is not definite. This is presumably due to the fact that a practice level for the general function was by this time near at hand. Support for this interpretation appears in the relatively small change in general level of efficiency from Day 6 to Day 15.

During Day 15 there is a small early decrement and no statistically certain practice improvement.

We may summarize our reading of these curves by saying that there was at the beginning of the experiment no certain work decrement, which fact was probably due to a masking effect exerted by factors making for practice improvement. As the experiment progressed and the factors making for practice improvement became less effective, a pronounced decrement appeared. This decrement

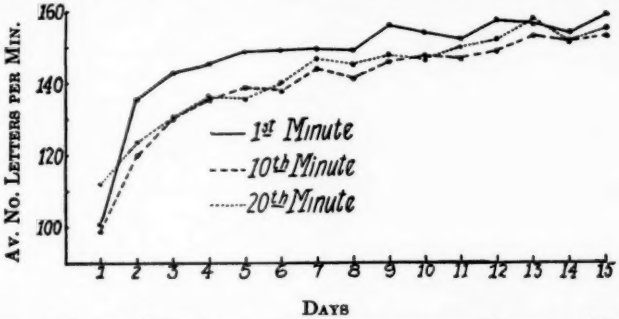

FIG. 2. PRACTICE CURVES FOR THE 1ST, 10TH, AND 20TH MINUTES OF WORK IN TERMS OF SPEED OF WRITING
(Average performances of 10 *Ss.*)

then diminished until at the end of the experiment it was considerably reduced. Whether a diminution in the causes of decrement was actually taking place even in the first days of the experiment can not be proved. It seems altogether likely that this was the case, however, and that the apparent increase in decrement was due more to the cessation of practice improvement (which always tends to mask decrement) than to any real increase in that decrement.

In Fig. 2 we have presented in different form the essential facts brought out above. We have plotted practice curves for the 1st, 10th, and 20th minutes of work for the entire experimental series. The amount of decrement within the work curve for any day is shown by the distance between the curve for the 1st minute and those for the 10th and 20th minutes. The small decrement for the 1st day is shown by the fact that the curves for the 10th minute and 20th minute are at that day about as high in level as that of the 1st minute. The diminishing decrement as the experiment progressed is shown by the converging of the three curves. This figure shows clearly that the tendencies brought out in our examination of the four sample work curves hold for the experiment as a whole.

In an earlier investigation of continuous writing it was noted that the writing increases in size during a period of continuous work.[2] It was not found, however,

[2]E. S. Robinson and A. G. Bills, Two factors in the work decrement, *J. Exper. Psychol.*, 9, 1926, 415-443.

that this increase influenced the magnitude of the decrement in speed. Work conditions showing relatively slight decrement manifested this change in size of writing as markedly as did those conditions in which the decrement was more pronounced. In spite of this fact we felt that changes in size of writing ought to be taken into account in connection with the present study.

In Fig. 3 we have presented results on the size of writing at three points in the work curve, the 1st minute, the 10th minute, and the 20th minute. Each curve in the figure represents those changes in size brought about by practice. The tendency of the writing to increase in size within the single 20-min. period is shown by the distance between the curves at any given day.

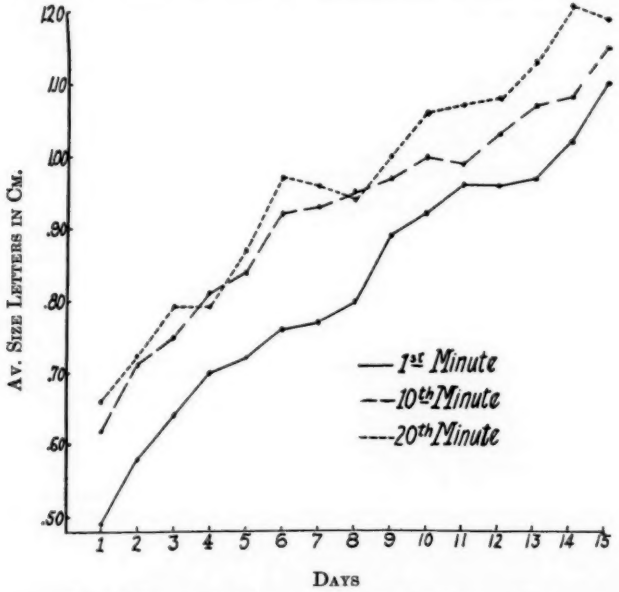

Fig. 3. Changes in Size of Writing throughout Practice (Curves represent average performances of 10 *Ss* for 1st, 10th, and 20th minutes).

As shown by all three of the curves, the size of the writing increases markedly with practice. The question is thus raised as to whether the decrease in speed decrement which also accompanies practice is a function of this other change, *i.e.* the increased size of the writing. If the intra-period change in size increased from day to day we should have direct evidence that this were the case. The change in size of writing from the 1st minute to the 20th minute, however, remains approximately the same throughout the experimental series and the change from the 1st to the 10th minute seems actually to diminish. It would appear, therefore, that such resistance to the speed decrement as develops with practice

is not due simply to an adaptation whereby the *S* comes to meet felt losses in speed by promptly increasing the amplitude of his movements.

We can hardly conclude that the larger size of the writing at the beginning of work on the later days has nothing to do with the increased resistance to decrement in speed. It is possible that with large early writing it is necessary to increase the size only moderately to protect against decrement. That is to say, an absolute increase which is insufficient to protect against decrement when the early writing is relatively small, may be sufficient when the early writing is large, as it was in the later days of the experimental series. It seems to us that this possibility is, however, only a remote one. In the investigation cited above it was definitely discovered that decrement in speed may be independent of changes in size of writing.[3] Since in the present experiment there is certainly no direct relation between intra-period changes in size of writing and the decrement, it seems likely that such changes in size as occur with practice do not constitute the principal cause of that reduction in decrement which also accompanies practice.

Summary

We have confirmed Glaze's finding that the decrement in speed which occurs in the continuous writing of *abab*, etc., for 20 min. is diminished by practice. We have considered another product of practice—the tendency to write larger and larger letters—but we have not found any direct evidence that this altered size of writing is an explanation of the diminished decrement.

[3] *Op. cit., J. Exper. Psychol.*, 9, 1926, 422.

HUNGER AS A FACTOR IN LEARNING

By W. A. Bousfield and M. Sherif, Harvard University

It is conventional to speak of the reflex as if it were a simple, point-to-point correlation between the stimulus and the response. At the same time it is practically taken for granted that most behavior which the behaviorist might want to call reflex, or at least to express in terms of stimulus and response, is dependent upon the general state of the organism, such as drive, fatigue, emotional factors, etc. In eating, for example, the hungrier the animal the less easily it is distracted from its eating; in general terms, the stronger the drive, the less easy is distraction. However, since the relation of stimulus and response forms the setting of most experimental work and theoretical behavioral discussion, it seems well to indicate the importance of what is more or less obvious to common sense by presenting definite quantitative data upon the eating reaction. These results also suggest a method by which drive can be measured.

The experiment consisted of the determination of progressive changes in the times of cessation of eating, after a loud noise, and when the animals had been deprived of food for determined intervals of time. The *S*s were 6 chickens and 6 rabbits.

The following technique was employed with the chickens. At 7:30 A. M. a supply of food, considerably in excess of what the chickens would eat at one feeding, was placed in the coop. Chicken No. 1 was removed and carried in a box to a point about a quarter of a mile distant, where it was placed in a cage also containing a liberal supply of food. At 8:30 A. M. the five remaining chickens in the coop were caged so that food could no longer be touched. The *E* then returned to Chicken No. 1 and waited, when necessary, for it to return to food. When it was evident that the eating was definitely under way, he fired a blank cartridge over the chicken's head, simultaneously with starting a stop-watch. All of *E*'s movements, as well as the sight of the pistol, were hidden by means of suitable screens. When eating was once more evident after the firing of the shot, the stop-watch was stopped, thus measuring the cessation time of eating in seconds. Chicken No. 1 was then allowed to remain in the cage for an additional interval of about 15 min. It was then removed, and after all remains of food had been cleaned up, Chicken No. 2 was placed in the cage. Following a fast of 1½ hours, food was presented to Chicken No. 2. The shot was fired as soon as the eating was definitely in progress, and the time of cessation was obtained with the stop-watch as in the case of Chicken No. 1. This procedure was repeated for the remaining four chickens, with periods of fast lasting 3, 4½, 6 and 7½ hours respectively. Since food was continuously present in the case of Chicken No. 1, the period of fast has been arbitrarily designated as 0.

The parallel experiment with the rabbits differed essentially only in one respect. The chickens were used on alternate days, and the rabbits on successive days.

*Accepted for publication March 16, 1931.

The results are shown in Figs. 1 and 2. In each figure the times of fasting increase from No. 1 to No. 6—for the six chickens in Fig. 1 and the six rabbits in Fig. 2.

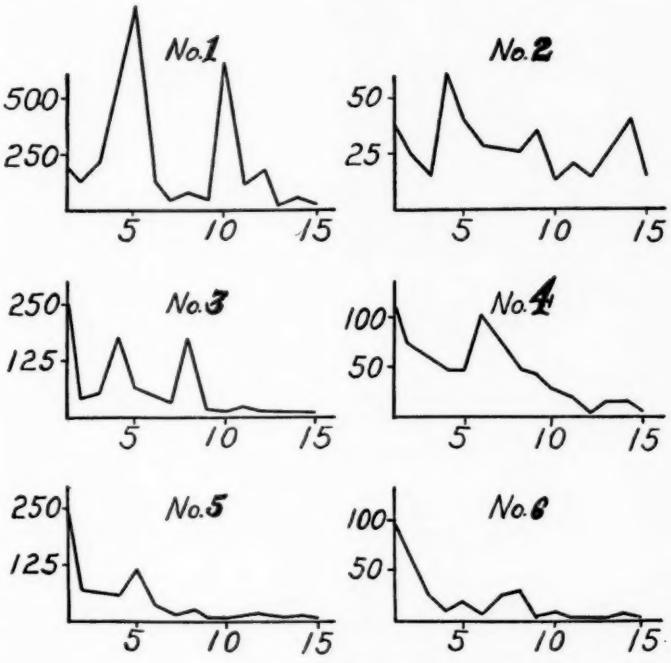

FIG. 1. RESULTS FOR THE CHICKENS
(Abscissae: Trials. Ordinates: Time, in sec., of cessation.)

It is evident that the times of cessation tend to decrease in successive trials, that the decrease is greater or more rapid or both as the period of fasting increases, and that cessation of eating at the pistol shot may fail to occur (as in the case of Rabbits Nos. 4, 5, 6, Fig. 2).

The figures suggest that the rabbits learned more rapidly than the chickens. However, it must be remembered that the rabbits learned on successive days so that there was less chance for forgetting. Moreover, it may have been easier for a rabbit to resume chewing upon food already in its mouth than for a chicken to initiate the entire pecking response.

Conclusion. The data of this experiment give an univocal quantitative indication of the influence of the condition of the organism as a factor in what may be designated as a type of learning. Specifically we may conclude: (1) that the

greater the degree of hunger, the sooner will a loud noise become relatively ineffective in inhibiting the responses to food; and (2) that the time of cessation of observable eating responses is a function of the number of trials in the cases of the longer fasts.

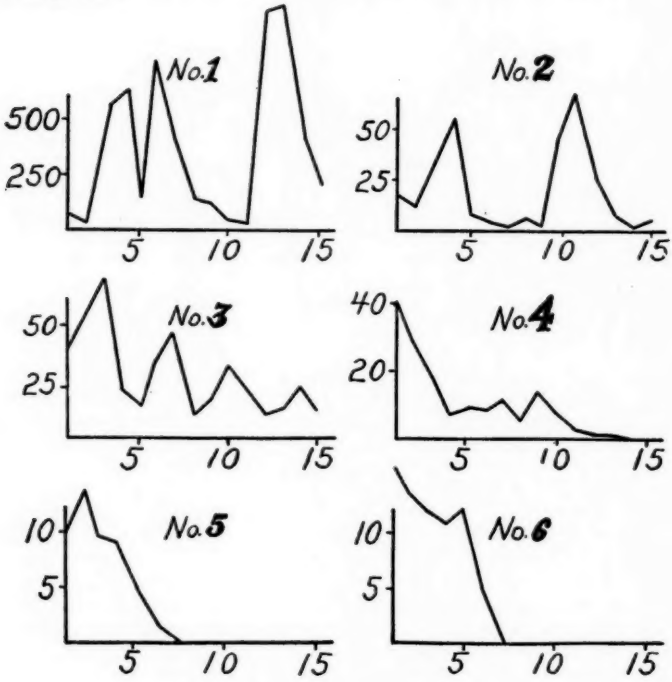

FIG. 2. RESULTS FOR THE RABBITS
(Abscissae: Trials. Ordinates: Time, in sec., of cessation.)

APPARATUS

THE CONTROL OF EYELID MOVEMENTS IN VISUAL EXPERIMENTS

By Sidney M. Newhall, Yale University

Blinking and winking, particularly during the course of visual adaptation or fatigue experiments, have not received the technological consideration that they merit.[1] This view was reached while reading the literature on visual adaptation and while trying to control the lid in adaptation experiments. In some reports of elaborate studies giving evidence of great precision in other respects, the matter of lid movement is either not mentioned at all or else dismissed in cursory manner. Even in studies which are not primarily adaptational, but in which the visual stimulus is of some duration, lid movement might prove disturbing. The considerable literature of these movements contains techniques for recording the blink or wink, results on the effectiveness of various stimuli and situations in eliciting the blink, correlations between movements of the eyeballs and the lids, data on the temporal relations of lid movements, and surprisingly little concerning devices for restraining the movements.[2] Consideration of such accumulated data in the light of the present problem may lead to improved control, and incidentally uncover results of more general interest. Some relatively simple control devices and certain other details which may be familiar to the expert are included for the possible benefit of others less oriented in this complex field.

First of all something should be said concerning the relation of the lids to the pupil. The entire pupil is unobstructed in the case of the normally open eye. The amount by which the lids clear the pupil depends partly upon its diameter, and that of course varies with the degree of light, and is affected by accommodation and emotion. Ordinarily, the lower lid is not particularly close to the pupil, and seems never to intercept normally entering light. The upper lid usually clears by a much narrower margin, and in a comparatively small number of heavy-lidded cases actually does partly cover the pupil. In the usual wink or blink the upward component of the lower lid movement is relatively slight, and probably never reaches the lower edge of the effective pupil. Therefore our especial concern in control is with the upper lid.

Synchronous with both reflex and voluntary lid action, however, there occur regularly movements of the eyeball which inevitably upset the line of sight.

[1]The term 'blink' is used to designate the reflexive lid closures while 'wink' is used for the movements made voluntarily or with intent.

[2]The relevant literature includes the citations by W. R. Miles, Elevation of the eye-balls on winking, *J. Exper. Psychol.*, 14, 1931, 311-332, and by J. Peterson and L. W. Allison, Controls of the eye-wink mechanism, *ibid.*, 14, 1931, 144-154.

Opportunities for coöperative research in the Institute of Human Relations were utilized in the preparation of this paper.

Miles recorded very rapid elevation of the eye simultaneously with the descent of the upper lid, followed by a slower reverse movement, as the lid was lifted. According to his measurements taken during the usual periodic reflex in reading or looking at a picture, the upward excursion of the cornea consumes about 50 σ and the downward about 110; while the total vertical displacement is in the order of 10 to 15° of visual angle. If such eye movement occurs only in conjunction with lid movement innervation, correction of the latter would eliminate fixation lapses due to the former. For the present, attention may be concentrated solely upon the problem of lid control.

The object in lid control in adaptation experiments is to minimize undesired interference with the incidence and effective duration of the experimental stimulus. A wink or blink in progress produces changes of the illumination situation in the region of the corneal surface. As angles change, light reflected from the lids varies, and there will be possible some change in the composition or quantity of flux entering the pupil. But it is the sudden reduction of the main experimental stimulus, with the resultant recovery from stimulation, which constitutes the most serious consequences of lid or ball movement. The duration of this cut-off may be important.

Actually the time relations of the lid movement are variable, both among individuals, and in the given individual. Consequently it is difficult to secure values which can be regarded as generally representative. The duration of the complete blink, however, *i.e.* from the commencement of closure to the end of opening, seems usually to be over 150 σ. In case of the common periodic closures during reading, Miles suggested 250 σ as representative of the time from the beginning of the movement until fixation is again established, although at 150 σ the cornea is back to within 2° or 3° of its original position. During the blink there is first the decreasing phase while the lid gradually covers the pupil, followed by the somewhat slower increasing phase while the lid rises at the end of the blink. These phases are somewhat protracted by the reverse ball movements already mentioned; in partial blinks the phases may be incomplete. Since the upper lid clears the pupil but slightly when the eye is normally open, during a full blink the pupil must be more or less obstructed for nearly the complete duration of over 150 σ, and for a certain part of the time the pupil is wholly covered, and the external light stimulus is largely cut out. Miles estimated this latter time to be in the neighborhood of 100 σ.

The wink is more variable than the blink because it is more affected by fatigue of the local mechanism and depends so much upon the *Aufgabe* of the *S*. Indeed, an individual may close his eyes at pleasure and hold them closed indefinitely; if it were legitimate to consider such actions winks, then winks would be indefinitely variable. The duration of a complete wink, according to Garten's average values, varies from 308 σ to 420 σ. He gives times for the lowering of the upper lid ranging from 75 σ to 91 σ, and times for the raising from 142 σ to 177 σ. Times of total coverage of the pupil are from 133 σ to 201 σ. The time for partial coverage is probably not much less than the total time of the wink, for the same reason as in the case of the blink.

This cut-out of stimulation by lid movement would not be a serious difficulty if the lid would leave the pupil clear for a duration sufficient to meet the requirements of adaptation experiments; the trouble is that the usual rates of blinking

are so high that a sufficient stimulation period cannot possibly be interpolated between two blinks. Table I indicates what short durations are likely to occur between normal, unrestrained, automatic blinks under 'ordinary' conditions.[3] Blinks were counted at all hours of the day and under a great variety of conditions, with the hope of getting something representative. The left column in the table gives the number of blinks per min.[4] The succeeding columns to the right show frequencies of occurrence of these different rates in the various *Ss* observed. *A*, for instance, blinked from 10 to 14 times per min. upon 28 of the occasions when his blinks were counted, 15 to 19 times on 52 occasions, and so on. It is clear that ordinary blinking will interfere with stimulation at frequent intervals.

TABLE I

FREQUENCIES OF BLINKS PER MIN. UNDER UNCONTROLLED CONDITIONS

Blinks per min.	Subjects						
	A	*B*	*C*	*D*	*E*	*F*	*G*
5–9						2	
10–14	28				3	11	
15–19	52	1		2	10	7	
20–24	36	8		3	12	7	2
25–29	15	19		8	7	6	8
30–34	11	49		8	5	3	6
35–39	3	36	2	6			7
40–44	2	12	4	2	1		6
45–49		9	8	3			9
50–54			12	2			1
55–59			11				
60–64			7				1
65–69							
70–74			6				

If the assumption is made that during the average blink the lids more or less cover the pupillary aperture for 150 σ, and if the *S* is blinking at the rate of say 20 blinks per min., then the stimulus is more or less reduced for as much as 5% of the total time. Along with this there is the doubt, discussed by Miles, concerning the adequacy of vision for 100 σ or so immediately following the lid movement. Therefore it seems not impossible that the lid movement may interfere for as much as 10% of the total time. Figures of this order in themselves point to the desirability of special control.

Though it is the retinal recovery due to lid or ball movements that concerns us especially here, we must note that those movements provide also for what Miles aptly terms the 'servicing' of the corneal surface, with the consequence

[3]All tabulated data are taken from unpublished studies by F. A. Almquist and the writer.

[4]A measure sometimes used is the 'inter-blink period,' and this is supposed to give the time elapsing between blinks. Unfortunately the measure is equivocal, since what it actually gives is the duration from a given phase of one blink to the corresponding phase of the succeeding blink; one or both blinks being actually in progress during the so-called inter-blink period. The measure is secondary in the sense that it is calculated on the basis of blinks per unit time.

of maintaining or restoring its condition of relatively uniform transparency in the region before the pupil. Both this corneal and the retinal recovery have a bearing upon the discrimination and acuity of vision which suggests a possible source of error needing to be investigated, viz., that when the eye is held open for a protracted period, what appears to be a consequence of retinal adaptation or fatigue may be in part a matter of mounting irregularities in the transmission of the optical system due to drying or particles collecting on the corneal surface before the pupil. Any such effect might be factored out by suitable experiments.

Of course, the control of lid movements is really justified only if they constitute a sufficiently large source of error. This they do if they create an error which is large relative to the other errors of experiment or to the differences investigated. What constitutes a significant error must depend upon the procedure and problem at hand. Blinking during dark adaptation, for instance, would not be as serious as during light adaptation. Blinking in the early stages of color adaptation, where the rate of adaptation is typically rapid, would probably have a more retarding effect than in the later stages. Blinks during a short period of adaptation might be more upsetting than in a long period. A satisfactory answer to the question of what is a significant error cannot be made in general terms, but a certain justification of the present paper may be offered and a warning sounded by presenting some data showing the magnitudes of effects ascribable to lid movements in some actual experimentation.

S fixated monocularly the center of a disk-shaped 3° field which was divided by its vertical diameter, the one half being of low purity and appearing pinkish, while the other half was neutral, though of equal brightness. *S*'s task was threefold: (1) to look at the center as steadily as possible; (2) to wink uniformly at rates presented by a metronome, but not to blink between times; (3) and to report any disappearances of the color apart from those synchronous with the wink itself. *S* estimated the fading out of the color by comparison with the adjacent neutral half of the field.

Different rates of winking were prescribed in order to discover whether or not there would result noticeable corresponding differences in degree of adaptation reported. One measure employed of the effect upon adaptation of the winking was the number of seconds elapsing before the color faded out. Table II summarizes results based upon experimentation with the left eye and the right eye during exposure periods of 5 and 10 min. for both. It shows that when *S* was winking at the rapid rate of once per sec., total de-saturation was not once

TABLE II

WINKS PER MIN. *VS.* ADAPTATION TIME

Winks per min.....	60	15	7.5
Av. adaptation time (in sec.) —		129	84

achieved; that when, in another series, the rate was reduced to 15 per min., the time of disappearance became finite and averaged 129 sec.; and that when the rate was dropped to 7.5 the time was only 84 sec. Unpublished data suggest that without any blinking or winking, under comparable conditions, the adaptation time would fall to around 40 or 50 sec. According to the measure here employed, winking seems to have had a pronounced effect upon the course of adaptation.

A second measure of the effect of winking upon the adaptation process was the number of disappearances of the color during the time of exposure. The color would rarely disappear for very long, and during the exposure period of 5 or 10 min. it might reappear and disappear a number of times. Table II simply referred to the initial disappearance. Table III shows the average numbers of disappearances when winking at the three respective rates. There were no fade-outs (*i.e.* disappearances) during the fast rate of winking, an average of 6 during the medium rate, and 15 during the slow rate. In other words, the more frequently the stimulus was cut off by winking, the fewer times the color experience reached zero. On the basis of this measure too, the rather small differences in winking would produce observable effects upon adaptation. Effects of reflex blinking would probably be of the same sort, though less in degree.

TABLE III

WINKS PER MIN. *VS.* FREQUENCY OF FADE-OUTS

Winks per min.....	60	15	7·5
Av. no. fade-outs	0	6	15

Again and again in the course of color adaptation experiments, various *S*s including the writer have observed and reported an obvious increase in saturation or brilliance following a wink or a blink. In some instances the color had already completely adapted out, and the wink seemed to provide the rest which again lowered the absolute color threshold; in other cases adaptation had been but partial, and the wink was followed merely by greater saturation or brilliance. Such introspective data seem to be unmistakable evidence for a degree of restitution ascribable directly to the interruption of stimulation by *a single lid movement*. Winking and blinking would seem to constitute a source of error sufficiently large to merit special efforts at control.

Procedural controls. Lid movements can be controlled to some extent without any special instrumental appliances. One simple expedient is to wink several times, or merely to sit with the eyes closed, for several seconds preceding the experiment, and to open them only just in time to begin. The voluntary closure is supposed to relieve the strain, make blinking less urgent, and so help prevent irregular lid movements. Sometimes it is feasible for *S* to determine himself the moment when the adaptation period shall begin, and thus derive maximum benefit from his preëxperimental rest.

A rather common practice is to have *S* wink at prescribed times during the adaptation period and try to refrain from blinking at other times. Prescribed moments for winking may be regularly spaced throughout the period and be made known to *S* by auditory or tactual signal. A variation is to present to *S* every x seconds two signals y seconds apart, and instruct him to make lid movements only within the y periods. This use of prescribed periods has the advantage of giving *S* time to make several winks together, which is often what he feels like doing. In any procedure where the winks fall within the adaptation period they should be made as uniform as possible so that the assumption can be made that they are having about the same duration.

Voluntary winking at scheduled times seems a fair control where the investigator will be satisfied with the relative values resulting; but if 'absolute' values

are needed, this method is not directly applicable. About the best that can be done with it is to have *S* wink at different frequencies in different series, and from the group of adaptation rates resulting, attempt to extrapolate values for an absolute curve.

Whenever *S*'s winking is regulated by a fixed time schedule, care is necessary to so regulate the spacing that he will be able to refrain from blinking for the duration of the non-wink periods. One might suspect that the individual's normal or usual rate, *e.g.* Table I, could be used in predicting this ability to refrain from blinking; but actually it does not appear to be true that the slower the blink rate the greater the ability to inhibit blinking. Indeed, there is some small evidence for a slight inverse relation between these two variables.

The direct thing to do in adjusting the length of the non-wink periods is to test the individual's ability to refrain from blinking under the given experimental conditions. Table IV exhibits average values for the non-blink span of 3 *Ss* taken at different hours of the day. The wide variations among individuals, and particularly the variation in the individual case, suggest the wisdom of

TABLE IV

MEAN NON-BLINK SPAN IN SEC.

(Taken hourly)

	A. M.					P. M.									
Subject	8	9	10	11	12	1	2	3	4	5	6	7	8	9	10
A	18	8	9	11	9	13	8	12	12	9	12	11	12	12	8
B	34	26	22	16	22	22	12	12	13	20	14	16	12	10	10
C	85	71	33	49	33	36	17	31	24	66	44	78	56	48	34

allowing a wide safety margin in setting up a time schedule for spaced winking. Winking times for *A* would clearly have to be much more closely spaced than for *C*. In the case of any *S*, ability to refrain from blinking is so variable that the arbitrary non-wink period must be made relatively short, *i.e.* as short as the shortest non-blink span, if all trials are to be counted. Longer non-blink spans than in the table are to be expected under favorable laboratory conditions, but the variability would remain high.

Another but related procedural control is merely to have *S* refrain from blinking for the whole of the experimental adaptation-time. This is probably the simplest and most satisfactory procedure of all when the duration and conditions are not such as to produce a disturbing degree of discomfort or a significant change in the optical properties of the corneal surface. Judging from Table IV, one might be inclined to say at once that *S* probably could not inhibit blinking regularly for a sufficient period to meet the requirements of any adaptation experiment. Still, due to the great variability of the span, almost any *S* will now and then succeed in refraining for a long period; if only such periods were counted, requirements might be met. This would be a wasteful expedient, but perhaps otherwise quite satisfactory.

The expedient of making the non-blink span of maximum length is a very practical one from the standpoint of adaptational technique, because, up to a certain point, the longer the mean span the larger the proportion of trials in which *S*

could keep his eye open for the required duration, and consequently the more efficient that phase of the procedure. Some of the controllable factors affecting the duration of the non-blink span are mentioned below.

The instructions and *S*'s reaction to them are of primary importance. Those *Ss* whose results are presented in Table IV, for instance, were asked to fixate steadily and to refrain from blinking "as long as possible." The last phrase permits of different interpretations; properly perhaps it should be taken literally to mean until the lids can no longer be held open; actually, one *S* might make the culminating lid movement as soon as discomfort became noticeable, while another might wait until he experienced severe pain or longer. As any one knows who has tried, the non-blink span is characterized by a mounting experience of strain or pain which in time reaches a more or less unbearable degree. The strain sensations may be wholly absent from the early part of the duration, but there is generally a progressive increase in the latter part anyway, sometimes with very rapid acceleration towards the very end. Discomfort is often intermittent, increasing and decreasing until the final intensive phase which always culminates in the alleviating lid movements. According to the writer's experience, some *Ss* make these movements sooner than necessary. While an inordinate degree of discomfort would naturally upset fixation and the experiment as a whole, a moderate amount probably does little harm.

The fact that athletes by habituation and practice develop marked ability to keep their eyes open in the presence of disturbing stimuli suggests the possibility of prolonging the non-blink span in that manner. The situation in the case of the adapted *S* is, however, quite different. Whereas the athlete becomes capable of continuous vision during a relatively brief, sudden, and intensive stimulus, as when the boxing opponent's glove passes close to the eyes, the adapted *S* must typically refrain from blinking during much longer periods of comparatively steady and moderate stimulation. Such observations as we have made suggest that mere practice in restraint is of little avail except in so far as it leads to the development or discovery of very special expedients, *i.e.* aids of the sort mentioned below.

In monocular experiments, relief may sometimes be had by closing the unstimulated eye. If there is enough difference in illumination this will change the diameter of the stimulated pupil, and might therefore require the use of an artificial pupil. Another expedient is to wink very slightly when the strain has mounted to an uncomfortable level. The objections to this expedient are that it must be highly practiced and repeatedly controlled to make sure the upper lid is not partially hiding the pupil during the wink. Then, too, the synchronous reverse ball movement might occur even though the wink were slight, and so upset fixation even though the pupil were not occluded. It is safer to have the *S* keep his eyes partly closed continuously, because then there is less chance of ball movement and it is so much easier for *E* to check upon the degree of closure. Holding the head tilted back somewhat during the adaptation period is nearly equivalent to partially closing the eyes; in both cases the exposed area of eyeball is decreased. One other expedient in prolonging the non-blink span is to voluntarily assume a visually relaxed attitude; for this, too, seems to retard the development of the strain sensations and sometimes reduces them when they are already present.

Even if the non-blink span has been prolonged as much as feasible by the use of various expedients, it still may not be long enough. There remains, as before suggested, the possibility of extrapolation. In different trials the adaptation times may be made of different lengths ranging from very short to the maximum of which the *S* is capable. If a sufficiently close relation is found between the measures of adaptation and the lengths of the adaptation periods, then values for still longer periods can be extrapolated. It is well known that extrapolated values are not necessarily dependable because of the sudden changes sometimes found in extreme situations.

A control which is simple, economical, and which appeared to give fairly satisfactory results in estimating rates of color adaptation, is described below. *S* adapts without any winking or blinking until the strain mounts to the point where he feels fairly certain that he is within a few seconds of his 'breaking point.' He then makes his comparison or match rapidly and thus brings the trial to an end. On different trials he will naturally be able to refrain from blinking for different durations, so that different adaptation times will eventuate, but time and amount of adaptation may be plotted against each other. In this way, *S*'s very longest non-blink spans will contribute to the data. If *E* finds that he is not getting enough shorter adaptation times for plotting purposes, he may instruct *S* to make some of his comparisons before he approaches his breaking point. The following scatter from the writer's data represents results secured with this control and the following procedure:

S, in a darkroom, faced the 3° vertically divided field aforementioned, and observed it in the Maxwellian view. This small field was surrounded by and concentric with a 30° neutral field. By means of two optical trains and two rotating disks bearing sectors of Munsell red 4-11, it was possible to adjust the brightness of the large field and both the brightness and purity of either half of the small central field.[5] On any trial, both large and small fields were first set at neutral with a uniform brightness of about 1.5 ml., and *S* was light-adapted. Then a degree of purity represented by 180° of red sector would be suddenly introduced by *E* into one half of the central field, and a color adaptation period would commence. As soon as *S* felt that his breaking point was imminent, he would turn a handwheel, rapidly raising the saturation of the erstwhile neutral half until it balanced the other half. The Y-axis in Fig. 1 shows the degrees of sector or quantity of color which had to be introduced into the neutral half to match in saturation the 180° half to which *S* had been adapting. The baseline represents the time of color adaptation in seconds. These are results of several weeks of experimentation, for only a few trials were feasible upon any given day. Each plotted circle represents a trial. A filled-in circle indicates expression of doubt by *S* as to the accuracy of his match on that trial. In view of the difficulties inherent in the rapid match process, and the fact that each individual trial is plotted, the trend of the data seems fairly definite.

Instrumental controls. There are a variety of controls which depend primarily upon some special device or apparatus. Some lie near the boundary between what we have called the procedural and the instrumental, but may be included

[5]The original apparatus, of which the present is an elaboration, was constructed and described by R. Dodge and S. M. Newhall, Colored after images from unperceived weak chromatic stimulation, *J. Exper. Psychol.*, 10, 1927, 2-6.

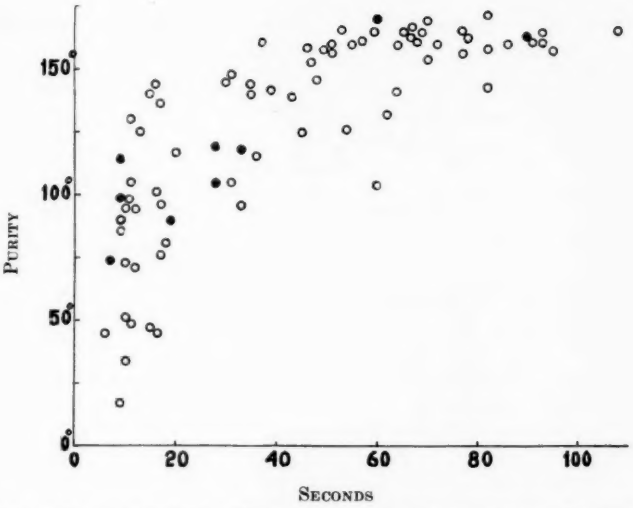

FIG. 1. RELATION BETWEEN TIME AND DEGREE OF ADAPTATION TO PIGMENT RED

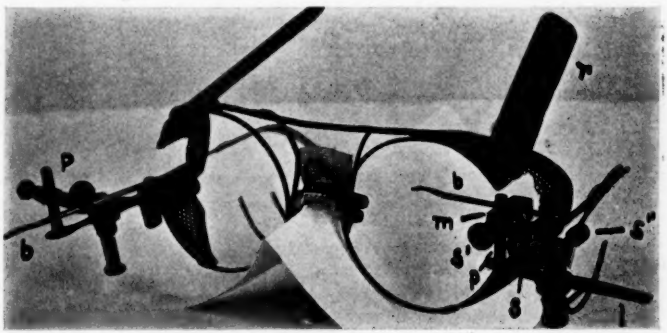

FIG. 2. THE OCULAR SPECULUM OR LID-HOLDER

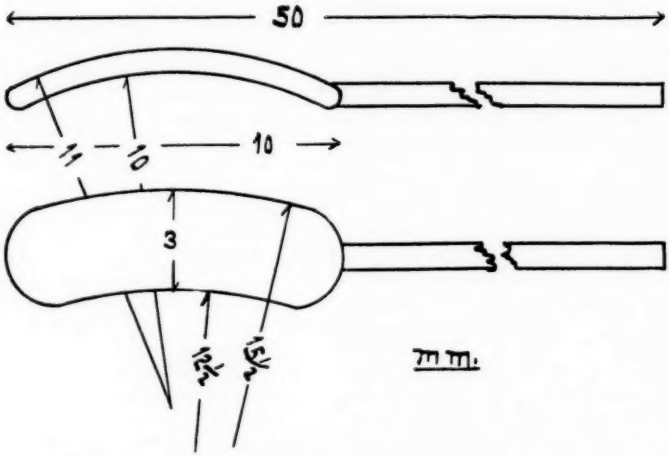

Fig. 3. Large Scale Drawing Giving Details of the Bar

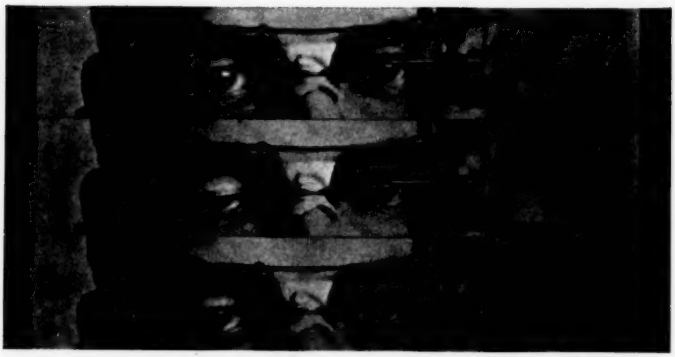

Fig. 4. Winking While Wearing Lid-Holder

here since, after all, the classification is quite arbitrary. Of these, the most effective means of eliminating lid movements is by drug injection which paralyzes the lid muscles. The levator which raises the upper lid, and the orbicularis which closes the eye, both must be rendered inactive. Drug injection is not to be recommended generally, however much it may appeal medically. It necessarily incapacitates *S* for periods before and after the experiment, it is inconvenient, particularly when *S* must serve repeatedly, and it interferes too much with the normal servicing of the cornea.

Drawing back the upper lid and holding it in place with surgeon's plaster is another simple means which is likely to suggest itself. But the repeated application of the plaster is somewhat trying and time-consuming, it is not effective unless drawn uncomfortably tight, and, unlike drug injection, spasmodic contractions may still occur and shift the position of the eyeball.

A more elaborate device for restraining lid movements, one which was designed particularly for that purpose, may be described in detail. This instrument might be called an ocular speculum or lid-holder, for it consists essentially of a small especially shaped bar for holding the upper lid clear of the pupil. This bar —one for each eye—is attached to a reënforced, blackened spectacle frame, and may be adjusted within the three spatial dimensions and swung quickly in or out of position. Ordinarily, for monocular experimentation, only the bar of the concerned eye is used. Fig. 2 shows the lid-holder complete, with one bar in the position it would occupy in holding the lid, and the other bar swung out of position. The large-scale line drawing in Fig. 3 gives the dimensional details of the bar for a right eyelid. It is made of brass and then given a coat of silver which is oxidized. This provides a surface which does not reflect much stray light, which is suitable for contact with the eyelid, and which can be cleaned with alcohol before use.

With reference to Fig. 2, the shaft of the bar, b, passes through a hole in the member m, and is held rigid in the hole by set-screw, s. With s loosened, the bar may be adjusted both laterally and rotationally. There is a second hole in m, permitting vertical adjustment upon the post, p. Member m may be set at any position on the post by simply tightening the set-screw, s′. That is the means for making the vertical adjustment of the bar. The post p is integral with a lever of the first class, l, by means of which *S* or *E* can swing the bar toward or away from the *S*'s cornea. On the side of the lever nearer *S*'s face, and bearing against the lever near the post, there is a third screw, s″. By means of this screw the closeness of approach of the bar to *S*'s cornea can be regulated. All three screws are provided with knurled heads, making a screw driver unnecessary, and adjustment by *S* himself possible.

Ordinarily *S* uses a mirror in adjusting the position of the bar. The instrument is first put on and the spring braces, r, set to give maximum rigidity of the frame. Then, with *S* looking straight into the mirror, the vertical adjustment is made by means of s′; next, by means of s, the lateral and rotational adjustments are completed; finally, with s″, the distance between the bar and the cornea is regulated. The positioning of the bar is of course all-important; it must be close to both the cornea and the projection of the pupil, clearing each by a millimeter or less. It should support the upper lid at the roots of the lashes. *S*, in making the last adjustment, holds his eye rather wide open and looks straight into the

mirror as he swings the bar toward the eye. But screw s' has first been turned toward the lever far enough to so limit its swing that the bar will not come very close to the cornea. Then, in a few successive trials, the screw is gradually turned back until at length the bar will just sufficiently clear the cornea when the lever is slowly swung up to contact with the screw. Once this stop-screw is properly set, the bar may be rapidly swung up to, or away from, the cornea in total darkness. To make the several adjustments quickly and satisfactorily is a matter of practice; if desired, all can be made without removing the spectacle frame. Good positioning becomes rather evident from the feel of the contacts. Two or three tentative winks will usually show whether or not the bar is close enough to the cornea to prevent the lid working down between them.

After the lid-holder has been adjusted, some objective test of pupillary clearance is desirable, under the experimental conditions and usually before and after the experiment itself. With the experimental intensity of illumination falling upon the eye, it is often sufficient for *S* to observe his own pupil in a mirror set up in the experimental line of sight at the proper distance. In some dark adaptation experiments it was found necessary to momentarily illuminate *S*'s eyeball in order that he might have sufficient light to observe the image in the mirror. Under such circumstances the bright light would temporarily narrow the pupil, and a greater clearance would have to be allowed between the lower side of the bar and the upper side of the pupil. Another case in which the bar must somewhat more than clear the natural pupil is when the stimulus subtends a large visual angle, for otherwise the bar might intercept the obliquely entering light. The appropriate use of an artificial pupil will sometimes render unnecessary the precaution of greater clearance. The bar itself may serve as mechanical support for an artificial pupil.

Trials of the lid-holder under experimental conditions indicate that if it is carefully adjusted it may be worn for as long as 10 min. without great discomfort. It has the advantage of affording a control not only upon the movement of the upper lid but also upon its resting position. Another advantage is that pre-experimental fatigue is minimized by the facility with which the bar can be swung into position at the commencement of the adaptation period.

The chief difficulty with the holder is that it cannot eliminate spasmodic contractions of the lid muscles. Even with the most careful adjustment these will occur at intervals throughout the period of restraint. If the synchronous ball displacements already referred to occur along with these spasmodic contractions, fixation is upset and the value of the holder may be considerably reduced. The point was tested by setting up a neon glow lamp in a dark-room at 5 meters from the *S*. *S* estimated the amount of displacement of the lamp image under various conditions, particularly when wearing the lid-holder or not wearing it. By reference to a series of vertical stripes placed 1° apart, *S* could make his estimates in angular terms. A few results from 5 *S*s seem to indicate that the image displacements in the case of the restrained lid movements were of the same order of magnitude as in the case of the full voluntary wink, *i.e.* somewhat less than 1°. This is a favorable indication because the displacements of the neon image must more nearly represent the full ball movement when the eye was constantly held open by the holder than when, in ordinary winking, the eye closed while still moving. If the image displacements were of the same order in both cases, the

actual ball movement when the lid-holder was worn was probably less than the movement during the ordinary unrestricted wink or blink. The holder, then, appears to reduce ball movement even though it cannot eliminate the muscle contractions.

But in view of the large displacements of 10° to 15° recorded by Miles during unrestrained lid movements, it seemed imperative to secure more objective information concerning displacement during movements restricted by the holder. Therefore several hundred frames of motion picture were taken during which *S* winked suddenly and rather vigorously a number of times.[6] These winks were at least as intensive as those which occur spasmodically while the lid-holder is being used. The film was examined under magnification to find frames in which winks had occurred. Since the left lid only was restrained by the bar, more or less droop of the right lid could indicate the crucial frames. That frame showing the maximum droop of the right lid caught by the 16 mm. camera, and the frames immediately preceding and following, were enlarged photographically, and sections showing the eyes are reproduced in Fig. 4.

The important point is the position of the eyeball in the middle frame as compared with its position in the preceding and succeeding frames. Measurements from the edge of the iris to fixed reference points on the lid-holder and forehead show no vertical displacement whatever! There does appear to be a slight nasalward movement. But allowing for error in measurement, any actual ball displacement here could hardly have amounted to more than 1° or 2°. According to this test the lid-holder not only restrains the lid satisfactorily but also reduces ball movement to the point where even relatively small stimulus areas are feasible. The latter is particularly true if there is not much retraction or backward displacement of the ball. There is known to be some backward displacement during unrestricted lid movements, but one cannot tell from the motion picture its extent when the lid is held open. The upward turn of the lashes in the middle frame indicates added pressure of the lid on the holder during the restricted wink. This and the somewhat smaller area of eye-white visible do make it seem probable that the ball has been pushed backward slightly.

The instrumental controls mentioned up to this point aim to actually restrain or render impossible the obstructing lid-movements; they are devices which more or less successfully prolong the non-blink span. There are, on the other hand, a number of devices which serve as controls by indicating or recording the lid movements when they do occur, but without interfering with them any more than necessary. Sometimes these may be useful for indicating slips in procedure, *e.g.* failure of *S* to keep his eye open during a non-wink period. Sometimes *E* may want *S* to blink normally during the course of the experiment and at the same time desire, either as a check on its normality or to discover what normality may be, a record of the blinking. There are, too, investigators who feel that no restraint or inhibition of the ordinary lid movements should be exercised during any visual experiments. They assert that the data are not typical unless secured when the organism is functioning naturally.

One of the simplest indicating controls is the signalling of *E* by *S* when the latter makes a lid movement. The signal may be executed orally, by the tap of

[6] This extraordinarily clear motion picture was taken by Professor H. M. Halverson.

a pencil, or by pressure of a key. In a series of observations in which the *S* could not know that his blinks were being counted, Almquist and I employed the silent key elsewhere described.[7] One *E* stationed beside *S*, and holding the key in his hand, depressed it at each observed blink. The other *E* in an adjoining room counted corresponding clicks of a single-stroke buzzer, the buzzer being connected in series with the key and a dry battery. Were the temporal spacing as well as the number of blinks required, the key could be connected with a kymographic marker instead of the buzzer.

In some of the observations just referred to, *S* was necessarily in darkness, and therefore invisible. Narrow strips of surgeon's adhesive tape were cut from a roll and coated with luminous paint. One strip was stuck lightly on the upper lid at the roots of the lashes, and the other was attached above the cheekbone. The luminous strips were visible in darkness, and change in the distance between them served to indicate lid movement.

Peterson and Allison state that "a little training enables one to count one's own blinks rather semi-automatically, using a stop-watch." We also found this to be a usable procedure. As already suggested, one person may count the blinks of another, either with or without his knowledge. In doing that, however, there is a source of error which might tend toward underestimation of blinking. The fact that the counter blinks as well as the *S* presents the probability that at some times they will blink simultaneously, or nearly so, and the *S*'s blink will not be seen. If the assumption be made that *E* and *S* both blink irregularly at a mean rate of 20 blinks per min., and that the blinks are about 200 σ in duration, then theoretically the probability of *any* degree of overlapping in the blinks of *E* and *S* is only about 0.0067. In other words, the two blinks could be expected to overlap more or less on an average of once every 150 blinks or once every 7.5 min. of adaptation time. The chance of sufficient overlapping to prevent *E* from noticing *S*'s blink might be even less, and in any event the resultant error would be negligible. This strengthens an observation of the authors cited above that when several counters are employed their results are in close agreement, and the accuracy of the method is demonstrated to be satisfactory.

There are a number of less simple techniques in which the lid makes its own graphic record. These appear less generally useful for adaptational purposes than controls previously described; but some of them may be mentioned. Exner's is among the earliest. He glued a thread to the lid and attached the other end to a delicate lever which wrote upon a revolving drum. Mayhew attached platinum foil to the lid in such a way that movement of it would break an electrical contact which would actuate a magnetic recording device. Cason used a combination of lever and electrical contact, while Peterson and Allison mounted a similar arrangement upon a specially devised spectacle frame. Garten photographed a spark displaced by lid movement. Miles photographed light reflected from minute mirrors, attached to the lids. Dodge took pictures of the excursion of an artificial lash of writing paper, 1.5 × 20 mm., affixed to the lid. He and Miles used a touch of gum arabic for attachment. Scientifically, the photographic techniques are the most satisfactory. The others set up situations so artificial that there is some question whether or not *S* becomes sufficiently adapted to them for such representative results.

[7] Newhall, A silencer for laboratory keys, this JOURNAL, 41, 1929, 646-7.

Stimuli to blinking. Various studies and common experience suggest that almost any intensive or unexpected sensory stimulus may elicit a blink. Consequently it behooves *E* who is trying to arrange for a long non-blink span or for regularly spaced winking to eliminate such stimulation. Ponder and Kennedy and others believe that even in the absence of special external stimulation the ordinary blinking goes on through automatic impulsion from a center in the striate body, unless, of course, it is voluntarily inhibited. According to Peterson and Allison, this blinking occurs even in the case of glass eye or blindness, and the spasmodic contractions continue for a time even after the eyes are closed. An obvious thing to do, then, in attempting to prolong the non-blink span, is to control the controllable stimuli and so allow *S* to concentrate voluntary restraint against these otherwise uncontrollable excitations. To this end something may be written concerning stimuli or situations about which something is known and which have likelihood of being encountered in visual experimentation.

The effect of type of activity of *S* upon the blink rate has been investigated. Almquist made about 40 observations each on 4 men students without their knowledge. Whenever he happened to be near one of these individuals the investigator counted the man's blinks and also noted the type of activity, viz. reading, talking, or simply sitting. The mean blink rates during sitting and talking were almost identical, but over 20% greater than during reading. Peterson and Allison also found blinking to be more frequent during 'not reading' or 'unoccupied' than during reading; indeed, of 5 activities studied by these writers, the highest blink rate occurred when *S* was 'unoccupied.'

Another of their results which is of considerable interest from the standpoint of visual experimentation concerns the distance of fixation. Seventeen *S*s gave a mean interblink period of 13.12 sec. with near fixation, as compared with 21.06 sec. during distant fixation. The corresponding values in terms of blinks per min. are 4.57 and 2.85 respectively; and their difference is statistically significant. It seems unfortunate that *S*s should have to blink so frequently during near fixation and when relatively unoccupied, because these are common conditions of the adaptation experiment. Possibly the adaptation procedure can sometimes be arranged to provide for distant fixation and some preoccupation of the *S*.

There is a paucity of data concerning the precise effects of simple sensory stimuli upon blink rate. Of all those that might affect blinking only one or two can be mentioned here. Light and change in intensity of light seem important because so frequently present in adaptational procedure. A simple test was made of the effectiveness of light, using 4 *S*s and the general procedure following. *S* was admitted to a darkroom and dark-adapted. Then for a period of 10 min. his blinks were counted in consecutive 15-sec. groups, using the darkroom technique for counting already described. During the first 2.5 min. of the 10, *S* was still in darkness with his head in a rest; during the second 2.5 min. he was exposed to a 6° disk-field of a brightness of over 1000 ml. and during the final 5 min. *S* dark-adapted.[8]

Fig. 5 exhibits average results based upon all the data; the ordinate gives frequency of the blinks per min., while the abscissa shows the three conditions just mentioned. The jagged lines connect the 15-sec. points and show the course

[8]Dr. T. W. Cook supplied the stimulation for this experiment, and also helped in other ways.

of change in blink rate during the experiment. Particularly interesting is the fact that the blink rate is high both at the beginning of the light period and the succeeding dark period; just why it should also be rather high at the beginning of the first dark period is not clear. though possibly that reflects some habituation to the headrest. In the beginning of each period there is a rapid fall in blink rate to a lower more stable level which persists throughout the remainder of the period. This means increased blinking on a sudden and considerable change in the intensity level of illumination, with consequent rapid habituation. The horizontal bars in Fig. 5 represent the general mean blink rates as under the three respective conditions. Evidently under the intensive light stimulation the

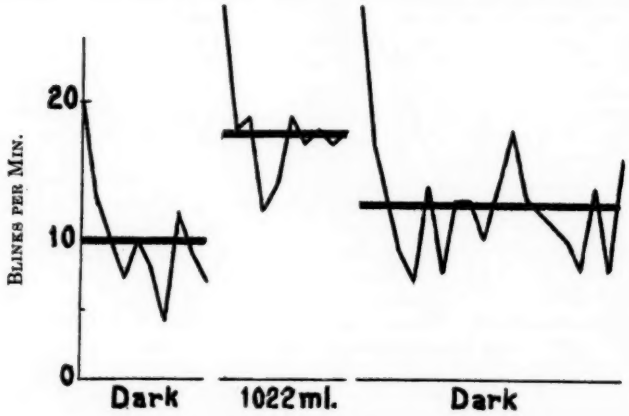

Fig. 5. Average Blinks per Min. during Extremes of Light and Darkness

blinking does not drop back to the average under the dark conditions, but maintains a higher rate for the 2.5 min. of exposure. Furthermore, the level during the final dark period has not in 5 min. reached that of the first dark period, though we should expect it to do so eventually. In general, the results are about what one might expect, but the data are insufficient to be more than suggestive. They suggest the wisdom of special vigilance in guarding against errors due to blinking in the early stages of light adaptation, the stage when it is progressing most rapidly.

Dryness of the cornea has been referred to repeatedly as characteristic stimulation for the reflex lid movement; but probably no more emphasis should be laid upon its efficacy than upon that of heat or many other factors. Some light might be thrown upon the question of the effectiveness of the aqueous condition of the outer surface of the cornea, it was thought, by studying the possibility of variation in the blink rate under various degrees of temperature and humidity. Exposure of the corneal surface to high humidity would naturally ensure greater moisture than on hot dry days. According to the old idea, if other things were equal there should be less blinking on the moist days.

A record was kept of the blinks per min. of one *S* on 20 different days, running over a period of weeks. Each day when the blinks were counted, the temperature of the surrounding atmosphere was taken in degrees Fahrenheit and the absolute humidity was measured in grains per cubic foot. The ranges of temperatures and humidities thus secured were not particularly wide; but the readings were taken in the laboratory and might be said to constitute working ranges in the sense of being values likely to be found during actual experimentation. The temperatures ranged from 60° to 77° and the humidities from 2.5 to 4.6 gr.

Table V gives the Pearson and partial correlation coefficients for blinks per min., (*b*), temperature (*t*), and humidity (*h*). Whereas blinking and temperature show an ordinary correlation of 0.64, blinking and humidity only correlate positively to the extent of 0.28. When the third variable is held 'statistically constant' by the conventional Yulean process, the one coefficient remains about

TABLE V

CORRELATIONS BETWEEN BLINKS PER MIN., ATMOSPHERIC TEMPERATURE, AND ABSOLUTE HUMIDITY

	b and *t*	*b* and *h*	*t* and *h*
Zero order	.64	.28	.53
Partial	.60	−.06	.47

the same while the other is reduced to around zero. Thus it would seem that coolness is more important than moisture. But these results are inconclusive and show no marked tendency in the ranges of variation ordinarily encountered in the laboratory building. If more cases and longer ranges were employed, it is possible that the coefficients would still hold their opposed signs and increase in size.

Air movement in the region of the eye seems to favor blinking and also tends to reduce the non-blink span. In one instance a rotating electric motor in some optical apparatus produced a slight draft in the vicinity of the cornea. Elimination of this air current seemed to produce increased comfort and greater ability to refrain from blinking. Similarly, air currents rising from a radiator and passing by the face may have a disturbing effect.

If there were a time of day during which the *S* did not have to blink as much as at other times, the *E* might be glad to know of it and to schedule his sessions accordingly. Neither in the case of the blink rate nor the ability to refrain from blinking have we been able to discover any marked tendency in that direction. Table IV above gives hourly values for the non-blink span from 8:00 A.M. to 10:00 P.M., and they indicate nothing definite save possibly some falling off from the first hour or two. A similar table, which there seemed to be no particular point in publishing here, shows the mean blink rates of the same three *S*s for each hour. The only indication is a slight and very irregular tendency for the rate to rise through the day. Further data might uncover something more definite; there may be optimum times for individual *S*s, or there is the possibility of rhythms in blink rate or non-blink span.

Concerning the question of choosing *S*s, a few words may serve to conclude these notes on lid control. Appropriate selection of *S*s contributes to control, as much as employment of special expedients and instruments. Whereas that

combination of procedural and instrumental controls which will be most appropriate depends very largely upon the particular problem at hand, the principles for selecting *S*s are relatively few and constant. Of course the investigator is after *S*s whose vision is satisfactory, and who do not blink often, or who can refrain from blinking for a long time. The great individual differences suggest at once the desirability of giving prospective *S*s a preliminary test to determine whether or not they are relatively proficient. The test should preferably be under the actual experimental conditions. There are *S*s to be found, Geldard says, who without much discomfort can refrain from blinking for minutes.

Other things being equal, it seems well to reject heavy-lidded candidates in favor of those who naturally keep their eyes rather wide open. If there is observable space between the upper lid and the top of the pupil, so much the better. This is to avoid the likelihood of the upper lid partially obstructing the stimulus, and to make the wearing of a lid holder, if necessary, more comfortable and effective.

Age and sex appear to have some influence upon blinking, as indicated in the results of Peterson and Allison. Blinking tended gradually to increase with age, and women seemed to blink more frequently than men, on the whole. Where otherwise feasible, this of course favors the use of comparatively young male *S*s. Emotional stress sometimes has a marked influence upon the blink rate; obviously, then, it is well not only to select *S*s, but also to arrange conditions in which such stress is not likely to arise.

A VARIABLE-PATTERN PAPER MAZE

By D. F. Showalter, Arkansas State College

The maze shown in the accompanying figure can be made on a mimeograph so that a large variety of forms may be produced at a small expense. The pattern shown is a ten unit, T-type maze with nine culs-de-sac. The correct path, which is shown as a solid line, is found by connecting the numbers from zero to sixty in order. The culs-de-sac are shown with broken lines. The path can be varied

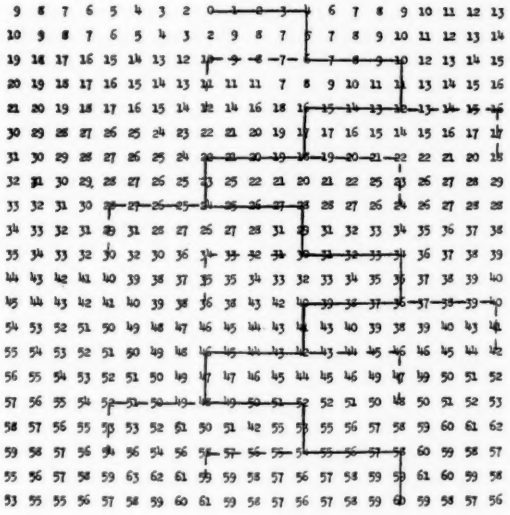

FIG. 1. THE VARIABLE-PATTERN PAPER MAZE

by changing the length of the lines, or the order of changes in direction, or by following an irregular route and returning to the starting point. A further modification may be produced by having either the letters of the alphabet in order or a single letter from a path through a maze of letters.

The path may be traced with a pencil or, if a carbon paper is used, with a glass rod. The glass rod leaves a faint mark which, though discernible, does not allow the pattern to be seen as a whole. This mark is entirely eliminated when the tracing is made of a sheet of transparent celluloid laid over the maze.

NOTES AND DISCUSSIONS

Are Emotions Teleological Constructs?

Nearly forty years ago William James married the emotions to the instincts so solidly that a divorce seemed to be impossible.[1] James' work was indeed but the giving of official sanction and due publicity to a union which had been tentatively announced by Charles Darwin in his *Expression of the Emotions in Man and Animals*. Apparently, the indissoluble bond has since been dissolved; but as a matter of fact, it is the death of one of the contracting parties that has broken the bond. Instincts have quietly passed away after a brief and feverish illness, and the widowed emotions have been left.

The detailed exposition of James resulted in a general acceptance of the scheme of emotions as organic results of responses. In so far as responses occur as unlearned patterns of specific and unique types, that is, as the manifestations of 'instincts,' we should expect 'emotions' of equally unlearned and unique types, capable of being named distinctively. In so far as responses are modified by the learning process, we should expect emotions correspondingly modified.

This analysis of the problem, indeed, seems as sound today as it did in 1882. If there is, for example, an instinct which may be called *flight* which manifests itself in different ways in different circumstances, but always in such a way that it is recognizable as *flight*, and definitely discriminable from any other instinct, such as the fighting instinct; then there should be a corresponding emotion, which we might call *fear*, unique in its nature and generically distinct from any other emotions such as 'anger.'

This doctrine, prior to 1918, was so well established that social psychology was said to be 'founded' upon it. It was so thoroughly ingrained into general psychology that the first behaviorism, although soft-pedalling on 'the emotions,' based itself upon 'the instincts' without an apprehensive question.

Then something happened. When I expressed before the American Psychological Association in 1919 my doubts concerning the reality of 'instincts,' my whole point seemed as fantastical to all but two or three of the members present as if I had expressed scepticism concerning the existence of spectroscopes. In a very few years, however, 'instincts' were banned from American psychology, and the revolt against instincts has even gone to the unwarranted extreme of putting a taboo on such perfectly good concepts as 'instinct' and 'instinctive,' which are really not bound up in the old 'instincts' at all.

Only a few psychologists, at the present time would base anything on 'instincts.' Apparently, the doctrine that 'an instinct' is nothing more than an arbitrary classification of responses in accordance with external results or external situations in which certain results *may* be accomplished, (*i.e.* a teleological classname); and that even the results by which classification is made are arbitrarily selected by the classifier; is generally accepted except by those who hold to a spiritual doctrine of 'instincts' as energies put out by a substantial soul.

[1]Presented before the Southern Society for Philosophy and Psychology, New Orleans, December 30, 1931.

After such a revolution, a logician might expect that emotions as uniquely different facts would have disappeared along with the 'instincts' to which they were obviously bound. This result, however, has not followed. Psychologists still talk of this 'emotion' and that 'emotion' as if they were *sui generis;* and the behaviorist has newly established behaviorism on the foundation of a few unique 'emotions' from which others are developed; the foundation which had been demolished by psychology being thus replaced by more of the same material.

I have from time to time suggested that the situation is illogical. I want to insist that it is absurd. 'Emotions' can be named: we can speak for example, of anger, fear, and grief. But these names represent no psychological entities, no unique affective processes. They are names applied to varying complexes in the affective content, and they are teleologically chosen. The search for 'primary emotions' is as much an anachronism in psychology today as is the search for the soul: and it is a search of the same sort. We must face the fact that the 'emotions' are names to which correspond no concrete realities, but designate abstractions of the most arbitrary type.

Let us consider 'fear' as an illustration. What is 'fear'? Its outward expression, we already realize, runs the gamut of responses. It may be manifested in flight; in immobility; in fighting; in caressing; in anything. On the other hand, the same outward expressions are characteristic of many emotional conditions. One fights when angry; when afraid; when in love. The responses to different external situations may become somewhat differentiated with advancing age: but the baby makes the same response to being dropped; to having a pin stuck in him; to being deprived of his bottle; to a violent shrill sound. No one but a behaviorist would be so rash as to conclude, with no evidence whatever, that the baby's 'emotion' is the same in all these cases. But, one will say with justice, these outward expressions are of slight consequence. It is true that fleeing 'fear' is different from fighting 'fear.' But the difference is unimportant as compared with the inner (bodily) states and processes. That, it will be said, is the same in all 'fear.'

Now just what *are* these inner states? I do not doubt that there are inner states: states of the muscles of the trunk, arms, limbs; states of the alimentary canal; states of the other tissues. I do not doubt that these states are experienced: that they are stimulus-patterns which initiate responses. But are they the same, or measurably the same, in all 'fear?'

So far as objective experiment goes, the answer seems to be: *No.* Further; the states or processes which do seem somewhat constant are also found in 'emotions' of quite different names. Shall we turn to introspection? Then the negative is still more emphatic. Different 'fears' do not feel the same. Not even 'fears' which arise in apparently similar circumstances.

So far as introspection goes, two 'emotions' of the same name are often more different from each other than they are from other 'emotions' of officially different names. This is not conclusive: introspection raises problems, never settles them. But the problem raised in this case will not down easily. If it is ever to be solved, experimental attacks must be devised to fit the problem, and not shaped along the lines of pseudo-problems. Emotion, however, is a fact quite aside from questions about 'the emotions.' We do get 'stirred up,' 'moved:' we *feel.* All introspection agrees on this point. What, then, do the names of 'the emotions' signify?

Nothing more than the situations in which we are emotional. In one circumstance, there is a threat or danger. Something, some person, some occurrence, threatens to limit me, to injure me, to deprive me of something I value. This is a definite constellation and the emotion I experience in this situation, I call 'fear;' although the actual emotion may vary. But hold, wait a bit: I do not always call it 'fear.' Sometimes I call it 'anger.' Yes; but then certain features in my environment are emphasized, which are less conspicuous in the situation in which I call my emotion 'fear.' If a person threatens me, and I emphasize his part in the portended happening I may still 'fear' him, if I admit his greater power. Or I may be 'angry' at him. It all depends on the way I think about the situation, not on the actual state of feeling. In still other ways of apprehending the situation I may call my emotion 'resentment.' In still others I call it 'grief.' It is not for nothing that we may sob in any of these cases and even in 'joy;' for 'joy' is merely the name we give to any emotion which occurs when we are specifically conscious of additions made to life. The vital differences, so far as feeling itself is concerned, are those between calm 'joy' and ecstatic 'joy;' cold 'grief' and frantic 'grief;' chilly 'fear' and frantic 'fear.' The differences otherwise are minor. Frantic 'fear,' frantic 'joy,' frantic 'grief' are much more alike, as emotional states, than frantic 'grief' is like to depressed 'grief,' frantic 'joy' to quiet 'joy,' or excited 'fear' to cold 'fear.' That in which the various 'fears' resemble one another, the various 'griefs' resemble one another, and the various 'joys' resemble one another, is in the likeness of the situations in which these states arise, or rather, in the perceptual and ideational apprehension of these situations.

To recapitulate: The emotions as classified in daily life, and in our archaic psychology are classified and named in accordance with our estimation of the situations in which they arise. I may estimate a situation today as different from an extremely similar situation tomorrow. Then I call 'the emotion' in the two cases by different names, corresponding to the situations as estimated or apprehended.

I may apprehend my situation primarily as one in which my welfare is threatened. I call my inner responses *fear*. You may apprehend my situation as one in which I fight against the threat; you say I am *angry*. Which is correct? The answer may be: Both! You name my emotion correctly in so far as you connect it with any factor in the environment which is really efficacious in stirring me up, or any actual intellectual attitude of mine towards the environment, or on any demonstrable result of my emotion.

It is no wonder that experimental attacks on the problem of emotions have produced no results. Our problems have been formulated in teleological terms, although we have supposed we were studying emotion as such. Our methods have been determined by our misconception of the problems. We have eagerly sought, for example, for characteristic organic symptoms of the conventionally named 'emotions,' overlooking the fact that the differences for a single so called 'emotion' should be expected to be as great as between two differently named 'emotions.' We have asked patient observers to report their 'emotions,' and have been misled by not knowing that the language in which the reports are given has mainly an environmental significance, and secondarily a cognitive reference.

In studies of the 'personality' type, we gravely record such statements of the patient as that he feels 'more cheerful' today; that he feels 'depressed,' or 'hopeful,' etc., never suspecting that the reports have little bearing on the actual affective state, but signify primarily the patient's judgment of situations. As indications of the patient's intellectual attitudes, these reports are of great value. Even the answer to the question: "Do you feel depressed?" may be significant, in spite of the fact that the same person will answer "yes" this week and "no" next week, but the significance is intellectual and not emotional.

I have been much impressed by the statement, in a recent report, of the method employed and results obtained in ascertaining employees' emotional states. The investigator first guessed the individual's state on the basis of his behavior. Then the individual was asked to report his state. If the two characterizations agreed, the rating was accepted as valid. If they disagreed, the individual was questioned later in the day, when it was usually found that his report agreed with the investigator's judgment!

In various ways, we find individuals tending to report their emotional states in terms of the ways they think they should feel in specific circumstances, as well as ways which they deem appropriate to the external conditions (which do not always amount to the same thing). Even when verbal report is ruled out, and other types of responses are interpreted, the interpretations are confused by the fact that the same response means different things at different times.

When a person is whistling lively tunes at his work, is he intrinsically 'cheerful'? Unless by 'cheerful' we mean merely that he whistles, the significance of his behavior is ambiguous. Often he is depressed. But usually, he thinks he should be cheerful, in a more inner sense, or that he should be optimistic, bucked up or looking on the brighter side. The determinants of classification seem in the predominant number of cases to be not mainly the actual inner feeling, but the external circumstance, and the individual's judgmental attitude toward the circumstances.

In another direction our confusion has been more deadly. We have, during the last two decades, more and more emphasized the emotional factor in life, at the expense of the cognitive. We have insisted, for example, that the perceptual and ideational qualifications of the child are not paramount: that his emotional attitudes in the home, in the school, among his mates, are of profound importance in determining his development, his learning, his adaptations. We have made a great pother about this discovery, in education and child guidance, and have created over night a lot of 'experts' in directing the child's emotional development. But when the results are checked, we find that little progress has been made, and that little in defiance of the theories. We are really dealing in this field with matters of understanding, judgment, and thinking generally. Solomon is justified in his maxim: practical success has been attained by getting the child to understanding his situation, to think accurately, and adopt healthy ideals. As we drop the ballyhoo about 'emotional attitudes' and face the problems as they actually are, we shall make still more progress.

There are emotional states or processes (whichever you choose to call them). These emotional items, moreover, actually do vary. Neither of these statements can be proved: but they are good working hypotheses. That there are different 'emotions,' discretely distinguishable in kind from one another, aside from their

cognitive associations, is a different hypothesis, which time has shown to be sterile. The more plausible hypothesis is that the variation is of the type which would occur in a complex in which there are n elements, any one of which may vary in a graded way, so that the total variation is poly-dimensional but without discriminable steps or jumps. The value of n, and the nature of the elements, if any, remain to be determined. It is safe to say, however, that if affective elements are ever discovered, they will not be items to which we would apply the conventional names of 'the emotions.'

It would seem that the best attack on the problem of emotion (not 'the emotions'), should be through the study of actual variations, regardless of emotional names. As a matter of fact, beginnings have been made in this line, It is unfortunate that we have to make these beginnings through classification; but if we hold resolutely to the fact that classifications are only classifications, we may even so make classification a useful servant.

We have noted that certain emotions, regardless of names, are pleasant, others unpleasant; some are exciting, others are the reverse. There are obviously dimensions of emotional variation which are by no means independent of external conditions and results, or of ideational processes; but which we can actually study as psychological features, collating them with cognitive factors, and not confusing them. Perhaps if we forget the conventional teleological classifications, and actually study their obvious dimensions, we shall learn something about them and discover other dimensions.

Such study as I urge must of course be psycho-physiological, rather than neurological. If we change the terms slightly, the James-Lange theory of 'the emotions' vanishes; but the modified James-Lange theory of *emotion* remains a permanent contribution to the armament of psychology. There is neither hope nor good sport in investigating emotions except as organic or bodily states: products of response to the environment, not intermediaries in response. The sooner we recognize that the traditional 'instincts' and traditional 'emotions' were really yoked together and that the passing of 'instincts' left 'emotions' so forlorn as to be hardly serviceable even for behaviorism and other reactionary psychologies, the sooner will our study of emotion become productive.

Johns Hopkins University KNIGHT DUNLAP

INSIGHT AND THE CONTEXT OF GESTALT THEORY

In the April number of this JOURNAL,[1] Ogden presents an attractive defense of the notion of insight from the standpoint with which he has identified himself. With the greater part of his discussion I am in substantial agreement. During the course of his exposition, however, he disposes very swiftly of ten points which I had raised in an earlier attempt to deal with some of the difficulties which the doctrine of insight creates for the middle-of-the-road psychologist.[2] A number of these questions, in fact, are summarily dismissed with the brief statement that they fall outside "the context of Gestalt theory" and are therefore meaningless, uninteresting to the configurationist, and devoid of fruitful

[1] R. M. Ogden, Insight, this JOURNAL, 44, 1932, 350-356.

[2] G. W. Hartmann, The concept and criteria of insight, *Psychol. Rev.*, 38, 1931, 242 f.

experimental possibilities. Since the limitations of space forbid detailed answers to all points, my reply will be confined to the more outstanding instances of disagreement.

As I see it, the major weakness in Ogden's position is expressed in the assumption that any critique of the Gestalt position, either in general or in detail, is null and void unless it first accepts the theoretical postulates upon which it is based. To maintain that a certain positivistically-worded logical or experimental problem has no connotation within the "context of Gestalt theory" is to evade the issue by merely denying its pertinence to psychology. The question is: Does the particular problem fall within "the context of psychology as such," or of "science as a whole," or even of "human experience in general"? No contemporary technician feels alarmed when one asks if the hydrogen ion concentration of saliva is raised or lowered during emotional stress, despite the fact that three conceptually distinct worlds of chemistry, physiology, and psychology are involved. Then why reject as illegitimate the perfectly harmless and rational first inquiry, "Are specific insights the outcomes of prior trial-and-error?"—especially when the contrasted principles of behaviorism and configurationism are both comprehended within the universe of psychology? To assert this seriously would work havoc with all efforts at reconciling opposing hypotheses within our discipline. Such a stimulating possibility as Watson's view that thought is simply the action of laryngeal mechanisms—to mention but one among thousands of similar 'fruitful' proposals—would never have appeared had Ogden's view prevailed. Surely no Thomist or existentialist could brush aside this question with impunity because it fell beyond the scope of his own private theory or framework of principles. Instead, the problem has generally been recognized as interesting and valid in its own right. Had I asked, "Do kittens write textbooks with left-handed clam shells?" then it would have been proper to say that this statement falls outside the "context" of everything except the universe of nonsensical language or of Alice in Wonderland.

Turning to the second statement, I believe the configurationists hold that *all* learning requires some insight—a bold assertion of universality which sorely needs confirmation. Insight implies a sense of the fitness and inevitability of the means for obtaining a solution. Yet what appropriateness does a child experience when it learns that pressing a button produces light—especially when the same effect can be secured by pulling a string or turning a wheel? Thorndike taught his cats to escape from confinement by scratching themselves. Can a more arbitrary or less insightful sequence be imagined? Nevertheless, learning in the broadest sense of attaining a goal certainly occurred.

With respect to the third item which concerns the best single criterion of insight, I am inclined to agree with Ogden that the discrimination of adequate means for attaining a desired end constitutes a valuable "earmark" of the process, although (to paraphrase another one of his statements) 'it presents more of a problem than a solution.' Numerous and disparate criteria of insight have been suggested as any casual survey of the literature shows. If that be the case, why is not the question of the most significant index of the process—as measured perhaps by the degree of correspondence between external event and inner experience—both conceivable and profitable?

My fourth question asked, "Is insight necessarily accompanied by ideas?" Ogden rejects this phrasing as implying a needless loyalty to an outworn creed,

but I see no reason why a catholic psychologist of today should hesitate to avail himself of introspective data in the interests of a richer description of the event, even if these accessories do fall outside the pale (or "context") of configurationism! The use of the term "ideas" is not intended to convey any theoretical bias; it is probably most safely employed in the general Lockian sense. What is said here applies also, *pari passu*, to Ogden's comments on the fifth and sixth items of my original list.

So far as the seventh problem is concerned, I am inclined to believe that a careful examination of *bona fide* cases of "association by similarity" may afford some of the finest testimony to the reality or unreality of the insight factor. Failure to appreciate this is very much like cutting off one's nose to spite one's face. A flat refusal to equate old and new terminology, just because the former does not fit the context of Gestalt, would involve turning one's back upon a great historical tradition, a step which Descartes could not literally take even though he tried. Most of us are happy to pour old wine into new bottles whenever an opportunity permits, as in Jaensch's suggestion that the eidetic image may have been genetically the transitional mediator between the "impression" and the "idea" (in Hume's sense).

The solution of some of these problems as I have stated them may demand a greater reliance upon narrower introspective procedures than many would be willing to allow. But the Gestalt theorists ought to be the last to protest on this score. Whatever differences there may be between introspective reports and phenomenological analyses (Wertheimer's historic paper of 1912 is a classic example of the latter) are probably not of sufficient import to affect the outcome of any investigation employing either or both procedures—or to prevent one from going ahead with the study.

Pennsylvania State College GEORGE W. HARTMANN

HELSON AND FEHRER ON "THE RÔLE OF FORM IN PERCEPTION"

Helson and Fehrer point out in a recent number of this JOURNAL,[1] that the problem of form has long been an important issue in theoretical and experimental psychology; but in recent times, this problem has assumed definite significance because of the work of the configurational school. This school emphasizes the view that perceptual and bodily responses of the organism *form* a whole, a unit, or configuration. Helson and Fehrer criticize this view, asserting that the configurationists have over generalized and over emphasized the rôle of form in perception. They also affirm, on the basis of their results, that circular forms are not preferred to squares and rectangles. Hence they are led to disagree with Köhler's hypothesis regarding preference for the 'simplest' symmetrical forms. According to their experimental findings, the perception of light at the absolute threshold for vision is formless, and only at higher levels of brilliance are forms just perceived; at still higher illuminations the certain form of the stimulus emerges.

This study stimulated the writer to reëxamine experimentally the problem of form. The writer is convinced that Helson and Fehrer's experimental work and theoretical conclusions leave much to be desired in a scientific attack upon

[1] H. Helson and E. V. Fehrer, The rôle of form in perception, 44, 1932, 79-102.

the problem. The evidence advanced by them that forms are not first or primary in perception seems inadequate and misleading.

If Helson and Fehrer's conclusions cannot be admitted, the problem remains, "What is the actual relation of the perception of form to the lower limen of vision?" Experiments carried out by the writer seem to answer this question. In the course of these experiments, it became evident that the perception of light at the absolute threshold for vision is *not* formless as Helson and Fehrer assert but that it is formed or organized in a whole with definite figure-ground properties. Thus, there is definite evidence that the correct explanation of the rôle of form in perception lies in quite a different direction from that given by Helson and Fehrer.

In the first of the writer's experiments, 4 *O*s were used. *O* was seated in a dark room before a test object in which different figures (squares; rectangles; triangles; L-shaped figures; jagged, irregular figures, and semi-circles) of the same approximate area had been cut. The figures were observed through a small artificial pupil. Four red dots served as fixation points. The visual angle subtended by the open figure fell well within the fovea. Monocular vision alone was employed. A reflecting screen was used which allowed independent illumination of the figures. Starting with an illumination which fell below the threshold of visibility, the brightness of the figure was gradually increased to a just perceptible brilliance. *O* was then instructed to draw and describe the appearance of the perceptual field. Invariably, the *O*s drew a lighted circular form on a dark ground, and described the figure as a circular area of light on a dark ground.

A modification of this experiment consisted in independently illuminating the area surrounding the figure. Here the figure was dark. In this case, the *O*s reported the appearance of a dark circular area on a lighted ground. There is thus positive evidence that light at the absolute threshold for visibility is 'formed' rather than 'formless.'

In another experiment, the surface of the test object surrounding the figure was illuminated and the brightness of the test object increased until the field appeared a uniform brilliance. It was then possible to investigate the emergence of a lighted figure on a ground of lower or higher brightness. The results of this experiment show that a figure emerging from a uniform ground of brightness appears first as circular in form. In still another experiment, the test object was greatly reduced in size by the use of a lens and *O* was required to draw and describe the appearance of the test object at the extreme limit of visibility. Again the figure was reported as a round lighted object on a darker or brighter ground. Further experiments, this time on the differential threshold for form where figure and ground were of different colors (hues), brought out the same results.

The assertion of the configurationists that forms are first and primary in perception finds experimental verification in this series of experiments. Helson and Fehrer appear to have confused form as assumed in the stimulus-pattern with form as it exists in the perceptual field. Figures objectively contoured and perceived as formed constitute only a small percentage of formed experiences. A bias of the *O* toward the stimulus-pattern (stimulus-error) —looking for 'objectified' form, rather than for form in the perceptual field as such—would explain Helson and Fehrer's results. The writer has been experimenting in vision intensively for several years and has yet to find in his own experience or that of his *O*s, an 'un-

formed' perceptual field. By no means will the form seen and the form of the stimulus-pattern always agree, a point the potency of which Helson and Fehrer have apparently overlooked.

University of Kansas WARREN W. WILCOX

"A QUESTION OF ETHICS:" A REJOINDER

In the April number of this JOURNAL,[1] Willis D. Ellis offers several criticisms of my introductory chapter to the *Listener's Notebook* prepared for the first series of broadcasts sponsored by the National Advisory Council on Radio in Education. Ellis' minor objection seems to be that my chapter contained erroneous generalizations. His major objection, however, is that such misstatements reflect not solely upon their author, but upon psychology and psychologists in general, since the radio pamphlet was, in a sense, an "official" publication. This misrepresentation of modern psychology he considers to be serious and unethical.

Ellis' objections are based upon a misinterpretation of what I actually said. On page 11 of my introductory chapter I wrote, apropos of several animal experiments which I had previously described, that "animal psychologists have found little evidence that animals in such situations 'reason' or 'think out' the correct solutions." Does this statement misrepresent present opinion among animal psychologists, as Ellis seems to think it does? As I am not a comparative psychologist, I shall cite from several recognized authorities in the animal field. In his radio address delivered on March 12, 1932, Thorndike says,[2] with reference to animal learning, "In such learning there need be, and usually is, no reasoning, no inferring, or comparing. There need be, and usually is, no thinking about things, no putting this fact and that fact together. There need be, and usually are, no ideas; the animal need not think of the box or the food, or the acts he performs. . . . . Animals learned by trial and success millions of years ago. Most of the animals in the world today do most of their learning in this way." Hunter,[3] after reviewing Köhler's ape-experiments, states his decision that "we are thus forced to the conclusion that no causal factor of insight has been demonstrated, and that to resort to such a hypothesis is to desert the straightforward experimental attack upon the determining conditions of behavior from which alone an adequate solution can be derived." Carr,[4] in his presidential address before the American Psychological Association, criticizes the view that insight is a method of learning separate and distinct from the so-called trial and error or motor method of learning in the following words: "Used in this way, the term insight will come to imply, I fear, some mystical, intuitive power of mind that unerringly, unfalteringly, and inevitably guides our mental footsteps in the paths in which they should go. To attribute this sort of insight to animals,

[1] W. D. Ellis, Psychology today: A question of ethics, *op. cit.*, 44, 1931, 365-367.

[2] E. L. Thorndike, Intelligence of animals and men, *Radio Lecture, Psychology Today*, (Ser. No. 20), 1932, 2.

[3] W. S. Hunter, *Foundations of Experimental Psychology*, 1929, 575.

[4] H. A. Carr, The interpretation of the animal mind, *Psychol. Rev.*, 34, 1927, 87-106.

is to endow them with powers not possessed by human beings. I am perfectly willing to humanize animals upon occasions, but I do most heartily object to deifying them."

These quotations are from men whose work, I believe, nearly every psychologist would agree is representative of modern trends in the comparative field. I leave it to my fellow psychologists to judge whether my sentence, as given above, is "unethical" in that it misrepresents modern psychology. What Ellis meant to say, I imagine, was that I had "misrepresented" his own personal views of animal learning.

Ellis' remarks directed against my treatment of the topics of the emotions and intelligence are interesting as examples of logic, but hardly worthy of comment. It would be easy to show, by citing authorities, that he is apparently not well informed on these topics. The examples of animal learning given above will suffice. Ellis seems not to recognize fully that I wrote these sections for the general reader without psychological training. That they are sketchy and leave much to be desired, no one appreciates more than I.

In conclusion, Ellis comes, it seems to me, very close to violating the "ethics" of fair play. A psychologist should at least acquaint himself with current knowledge in the field before he undertakes to inform—or reform—other workers.

Columbia University HENRY E. GARRETT

FALLIBLE SCORES AND THE STANDARD ERROR OF A DIFFERENCE

It is desirable occasionally to determine the significance of a difference between two 'true' measures, one or both of which have been determined from fallible scores. The usual formula for the standard error of a difference is inadequate for this purpose. There are four possible types of differences which may arise, namely: (1) when one variable has been measured by 'true' scores and the other by fallible scores, but the variables are uncorrelated; (2) when one variable has been measured by 'true' scores and the other by fallible scores but correlation exists between the variables; (3) when both variables are measured in terms of fallible scores, but the variables are uncorrelated; and (4) when both variables are measured by fallible scores and correlation exists between the variables.

The standard error of a difference between two measures when they are uncorrelated is

$$\sigma_d = (\sigma_1^2 + \sigma_2^2)^{\frac{1}{2}} \quad [1]$$

In many problems in education correlation exists between the two variables, which as pointed out by Walker may exert a not inconsiderable influence on the result.[1] When correlation exists between the measures formula [1] becomes

$$\sigma_d = (\sigma_1^2 + \sigma_2^2 - 2\sigma_1\sigma_2 r_{12})^{\frac{1}{2}} \quad [2]$$

Now if the variables are perfectly reliable r_{12} will equal $r_{\infty\omega}$; σ_1 will equal σ_∞; and σ_2 will equal σ_ω. If, however, either or both variables are not wholly reliable, the attenuation in σ_1, σ_2, and r_{12} must be considered. We need now, before we can proceed, the true standard deviation, the correlation coefficient corrected for attenuation, and finally the correlation coefficient corrected for attenuation in one variable.

[1] H. M. Walker, Concerning the standard error of a difference, *J. Educ. Psychol.*, 20, 1929, 53-60.

The formula for the true standard deviation has been given by Kelley[2] as:

$$\sigma_\infty = \sigma_1 (r_{1I})^{\frac{1}{2}} \quad [3]$$

Spearman[3] gives the correlation corrected for attenuation as:

$$r_{\omega\infty} = r_{12}/(r_{1I}\, r_{2II})^{\frac{1}{2}} \quad [4]$$

The formula for the correlation corrected for attenuation in one variable as given by Kelley[4] is:

$$r_{2\infty} = r_{12}/(r_{1I})^{\frac{1}{2}} \quad [5]$$

We may now by appropriate substitutions in formulas [1] and [2] secure the desired standard error of a difference.

The standard error of a difference between a true and a fallible measure when they are uncorrelated is given by formula [6].

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2)^{\frac{1}{2}} \quad [6]$$

If correlation exists between the two variables then formula [6] becomes:

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2 - 2\sigma_1\sigma_2 r_{12})^{\frac{1}{2}} \quad [7]$$

We now desire the standard error of a difference when both the original measures are fallible. This is given by formula [8] when correlation is absent and by formula [9] when correlation is present.

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2 r_{2II})^{\frac{1}{2}} \quad [8]$$

$$\sigma_{td} = (\sigma_1^2 r_{1I} + \sigma_2^2 r_{2II} - 2\sigma_1\sigma_2 r_{12})^{\frac{1}{2}} \quad [9]$$

It should be noted that in all cases the standard error is reduced, but that as the reliability coefficients approach unity, the values given by formulas [6] and [8] approach that of formula [1], and similarly the values given by formulas [7] and [9] approach that given by formula [2].

Fordham University JACK W. DUNLAP

THE NEW YORK MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The twenty-eighth annual meeting of Experimental Psychologists was held at Columbia University on March 24 and 25, under the chairmanship of Professor R. S. Woodworth. Nineteen members from fourteen universities were present and reported upon the work in progress in their laboratories. The 1933 meetings will be held at the University of Pennsylvania under the chairmanship of Professor S. W. Fernberger.

K. M. D.

THE ACCURACY OF PHASE CYCLES IN A COMMERCIAL ELECTRIC CURRENT

In a recent experiment we ran the constant speed motor of a Dunlap Chronoscope by a 60 cycle A.C., current supplied by the Philadelphia Electric Company. The current is now employed so widely for use with telechron clocks and with synchronous motors used for other purposes that great efforts are made to maintain a constant rate of alternation. On the basis of the current which we are using

[2]T. L. Kelley, The measurement of overlapping, *ibid.*, 10, 1919, 459.

[3]C. Spearman, The proof and measurement of association between two things, this JOURNAL, 15, 1904, 88.

[4]Kelley, *op. cit.*, 460.

the normal reading of the single unit of the Dunlap Chronoscope measures 1/1200 sec. It has seemed worth while to determine the accuracy with which the electric company maintained the cycle ratio.

The electric company very kindly supplied me with graphic records of their cycle rate covering the period of an entire week. The records show that the cycle rate was maintained within an error of 1/10 cycle with a high degree of accuracy. This means that if one were measuring reaction-times of the magnitude of 200/1200 sec. the range of accuracy would be between 199.8/1200 and 200.2/1200 sec. During the week the accuracy deviated more than 1/10 cycle only 23 times. The deviation was never as great as 1.5 cycle. The large deviations are of only short duration and apparently are due to sudden changes in load. Hence the largest deviation would give an accuracy of from 199.6/1200 to 200.4/1200 which is below the sensitivity of the Dunlap Chronoscope and certainly below the sensitivity of the accuracy required of practically all chronoscopic work in psychology.

University of Pennsylvania SAMUEL W. FERNBERGER

Harald Höffding: 1843-1931

The Danish philosopher, Harald Höffding, was born in Copenhagen on March 11, 1843. He passed the state examination in theology at the University of Copenhagen in 1865, took his doctor's degree in philosophy in 1870, was habilitated as *Dozent* in philosophy in 1880, and was made Professor of Philosophy in 1883. In 1914 he was elected to the honorable residence of Carlsberg, founded for the most eminent Danish citizen. In 1915 he resigned his professorship. He died on July 2, 1931.

The main trend of his philosophical development was directed away from theology toward a naturalistic philosophy, with a positivistic and critical (Kantian) influence from Sören Kierkegaard, Comte, and Stuart Mill.

His psychology is a "psychology without soul" which has as its main object the mental phenomena, and which regards introspection as it chief method. He regarded the experimental method with friendliness, but thought it applicable only to the most elementary phenomena.

Höffding was a thorough determinist. Concerning the relation between body and soul he maintained empirically the identity hypothesis: the mental and the material world are two manifestations of one and the same being, both given in experience. Metaphysically he was inclined to maintain (with arguments from the theory of knowledge) that the *mental* stands closest to the innermost essence of existence.

The main content of his psychology resembles the older association psychology. Höffding thought that he had avoided atomism by regarding the so-called mental elements as abstractions, and by considering the mental life as having an active and a passive side. "The passive side corresponds to the diversity of elements, the active side to the unity of all conscious content, whereby the individual elements are connected and brought into interaction. Kant, therefore, rightly characterized consciousness as a synthesis, which always presupposes a given manifold and combines individual sensations into percepts, forms representations into concepts, and so forth." A special case of this synthesis appears in his law of relativity: "From the moment of its first coming into being, the existence and properties of a sensation are determined by its relation to other sensa-

tions." Among the three mental elements, feeling, cognition, and will, he regarded the will as the most fundamental, and as identical with the activity of the synthesis which in every individual case is directed to a definite, more or less conscious, end. As the will is not a single element, but the form of conscious activity, it is not directly observable. The association of ideas by contiguity, similarity, and contrast are regarded as forms of the fundamental law of totality.

Höffding's main contribution to psychology is the emphasis he gave to *recognition*. His "quality of recognition" (*Wiedererkennungsqualität*) points, by way of the *Gestaltqualität* of Ehrenfels, to more recent developments in psychology.

His *Outlines of Psychology*, which first appeared in Danish in 1882, has gone through many editions, and has been translated into many languages (English, 1891). A part of its success is ascribable to the prominence of more philosophical analyses, which makes it a sort of *Einleitung in die Philosophie*.

The other writings of Höffding contain a large amount of valuable psychological reflections. This is specially the case with his *Ethics*, *Philosophy of Religion*, and *Human Thought* (his main work upon the theory of knowledge). His little book, *The Great Humour*, gives an interesting psychological and philosophical analysis of his own fundamental emotional attitude towards existence.

A bibliography by K. Sandelin of all Höffding's writings of psychological interest will be found in the third volume of the *Psychological Register*. A bibliography by the same author of all writings of Höffding, and most of the writings about him, will be found in *Harald Höffding in Memoriam* (Copenhagen, 1932), which also contains speeches about Höffding by E. Rubin, J. Jörgensen, V. Kuhr, and F. Brandt.

Copenhagen EDGAR RUBIN

Max von Frey: 1852-1932

Max von Frey, the sponsor of the classical theory of cutaneous sensibility, died at Würzburg on January 25, 1932, at the age of 79. He was born at Salzburg, November 16, 1852, a close contemporary of Stumpf, Ebbinghaus and G. E. Müller, but twenty years younger than Wundt. Von Frey went as a student to the universities of Vienna, Leipzig, and Freiburg, and was awarded the medical degree at Leipzig in 1877. In 1882 he was habilitated at Leipzig as *Dozent* in physiology, where the great Ludwig was professor, and he was made *ausserordentlicher* Professor in 1891. In these years his research had to do with the circulatory system, and he published three small monographs on this subject-matter in 1876, 1880, and 1892.

Von Frey's psychological interest in cutaneous sensation began in his later years at Leipzig. In 1894 he published *Die Gefühle und ihr Verhältnis zu den Empfindungen*, a twenty-four page brochure, in which he argued that sensory pain is the unpleasantness of feeling and that pleasure is not a feeling nor a sensation but the surcease of pain.

It was at the same time that Von Frey published his classical papers on cutaneous sensation. There were in 1894-97 four short notes, totalling fifty-two pages, in the *Berichte der königlichen sächsischen Gesellschaft der Wissenschaft zu Leipzig*, and the long paper on pressure and pain in 1896 in the *Abhandlungen* of

the same society.[1] It is here that he took over the discoveries of Blix and Goldscheider about the sensory spots on the skin, verified the results, established the lesser frequencies of Blix as against the greater frequencies of Goldscheider, set up pain as coördinate with the other three modalities (pressure, warmth, and cold), invented the hair esthesiometer, showed that the true pressure stimulus is proportional, not to weight nor to pressure per unit area, but to pressure per unit of linear dimension of a small stimulus-surface, created from this finding the surface-tension theory of pressure-stimulation, noted the very exact relationship between the location of hairs and of pressure spots, and finally hunted and argued out a separate nervous end-organ for each of the four modalities (the free epithelial endings for pain, the corpuscles of Meissner and the free endings at the hair bulbs for pressure, the end-bulbs of Krause for cold, and the Ruffini endings for warmth). Not all of these findings were to remain accepted truth, and the correlation of cutaneous qualities with nerve-endings doubtless persisted much longer than it should solely because of the prestige of Von Frey.[2]

It is not clear what turned Von Frey to sense-physiology. The atmosphere at Leipzig was more psychological than at any other university. Wundt's laboratory was the psychological center for the whole world. Yet Von Frey never published in Wundt's *Philosophische Studien;* nor was he one of the psychological physiologists of the camp opposed to Wundt and Leipzig—König, Aubert, Exner, Helmholtz, Hering, and Von Kries—who participated in editing the *Zeitschrift für Psychologie.*[3]

Von Frey went from Leipzig to Zurich as *ordentlicher* Professor in 1898, and then a year later to Würzburg, where he remained the rest of his life. During his first decade at Würzburg he put out half a dozen papers, two of them with Kiesow, on the end-organs in the skin. He had acquired some interest also in the problems of cutaneous space, and with Metzner published his well-known study of the successive two-point limen.[4] Brückner's study of the simultaneous limen also was made under Von Frey's direction.[5] In 1904 he issued his textbook, *Vorlesungen über Physiologie*, which went into a second edition in 1911, and into a third in 1920.

During the last two decades Von Frey has been the author of almost two score of short papers on cutaneous, kinesthetic and organic sensations, their end-organs, and cutaneous space and movement. Almost three-fourths of these publications have been in the *Zeitschrift für Biologie*, and the whole group is almost unknown to psychologists. During all this time Von Frey maintained his original faith in the

[1]The classical references may be given in full, since they seem not to be thus given in any standard secondary source: M. von Frey, Beiträge zur Physiologie des Schmerzsinns (i), *Ber. d. kgl. sächs. Gesell. d. Wiss. zu Leipzig, math.-phys. Cl.*, 46, 1894, 185-196; *idem* (ii), *ibid.*, 283-296; Beiträge zur Sinnesphysiologie der Haut (iii), *ibid.*, 47, 1895, 166-184; *idem* (iv), *ibid.*, 49, 1897, 462-468; Untersuchungen über die Sinnesfunktionen der menschlichen Haut: I. Druckempfindung und Schmerz, *Abhandl. d. kgl. sächs. Gesell. d. Wiss. zu Leipzig, math.-phys. Cl.*, 23, 1896, 169-266.

[2]Cf. K. M. Dallenbach, The temperature spots and organs, this JOURNAL, 41, 1929, 344.

[3]Of course Von Frey could hardly have participated in the founding of the *Zeitschrift* in 1890, while he was still known only for research on the vascular system.

[4]M. von Frey and R. Metzner, Die Raumschwelle der Haut bei Successivreizung, *Zsch. f. Psychol.*, 29, 1902, 161-182.

[5]A. Brückner, Die Raumschwelle bei Simultanreizung, *ibid.*, 26, 1901, 33-60.

punctiform distribution of sensory spots on the skin, and sought to show how many perceptual complexes were to be accounted for on this basis. Thus in 1923 he contributed to the *Festgabe* for Von Kries a paper in which—since the *Festgabe* was an issue of the *Psychologische Forschung*—he most appropriately aligned himself with *Gestalt* psychology.[6]

The *Psychological Index* lists 38 articles by Von Frey alone and 14 more in which he collaborated with another author. Of this total of 52, there were 14 in the period 1894-1910 and 38 in 1911-1930. It is clear that the psychological bibliography of Von Frey, the physiologist, is considerable. All these papers deal with somesthetic sensation and perception, except for one article on smell and another on sleep. In general, however, Von Frey lost contact with the psychological world. Nevertheless the psychologists will honor his memory as the E. H. Weber of the latter nineteenth century. No well educated psychologist can ever draw his pen across the back of his hand and feel the little thrills of cold without thinking of Von Frey, even though Blix discovered the spots, even though there are no Krause end-bulbs there. E.G.B.

[6]Von Frey, Über Wandlungen der Empfindung bei formal verschiedener Reizung einer Art von Sinnesnerven, *Psychol. Forsch.*, 3, 1923, 209-218.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Reflex Action: A Study in the History of Physiological Psychology. By FRANKLIN FEARING. Baltimore, Williams & Wilkins Co., 1930. Pp. xiv, 350.

In an introductory chapter, this general problem of the book is set: "Is the body animated by an immaterial agency, called 'soul or spirit,' or does it run like a machine, the functions of which are determined by the parts?" Thus to state the problem in terms of a dualism from which, by the way, the author never extricates himself, seems particularly unfortunate. A modern experimental psychologist would be embarrassed to choose between these alternatives, because both are obviously wrong. A scientist would prefer to remain uncommitted to any preconceived solution. The concept of reflex action, particularly as conceived by Pavlov, is at once introduced, since Pavlov's "position is typical of current physiological theorizing," and some of the objections to this solution are briefly indicated. The current mechanistic-reflex theorizing regarding the nature of the higher integrations of animals and man is interpreted as "a reaction against the older dualistic and animistic conceptions involving mind, will, or any type of conscious process" (p. 3). The content of the volume and the statement that "The story of the development of the concept of reflex action and of the phenomena on which it is based, is the object of the present historical survey" (p. 7), show that the author is seeking to study the reflex mechanism in a very wide sense. But why then are such significant investigators as Cannon, Carlson, Coghill, *et al.* omitted from modern contributors? If the present study is particularly interested in how "the body (is) animated" what is the reason for the neglect of the work on autonomic mechanisms and activities? On the other hand, the old-time classifications of voluntary and involuntary movements are frequent throughout the book, and the author seems to identify the 'involuntary' with reflex movements. There is always a sort of implication that 'voluntary actions' are run by something else than a stimulus—*e.g.* by 'consciousness,' 'mind,' or what-not. The author never refers to literature which attempts experimentally to throw light on 'volition;' nor does he attempt himself to clear up this unexplored something to which one class of actions is constantly referred. It is, of course, to be conceded that complete consistency and inclusiveness cannot be expected in a book like this one; but we intend here only to indicate what seem to be some weaknesses. Clear definition and distinction of terms are scarce indeed; and mysticism is an ever-present menace.

After brief indications of some pre-scientific conceptions of neuro-muscular action, the author divides the historical development of the reflex arc concept into the following stages: (1) the pre-scientific period, before the time of Descartes in the 17th century; (2) the speculative period, ending with the appearance of Robert Whytt's book on the *Involuntary Motions of Animals;* (3) the period of nascent experimentation (1750–1831), ending with Marshall Hall's work on

the spinal cord reflexes, 1832; (4) the period of increasing knowledge of the structural components of the reflex arc (1832–1906); and (5) the modern period, dating from the publication of Sherrington's *Integrative Action of the Nervous System*. Developments in each period are treated in considerable detail. Chapters 2 and 3 on the 17th century developments have earlier been published in the *Psychological Review* and in this JOURNAL, respectively. Chapter 13, *The Reflex Maintenance of Posture*, has been published in the *Journal of Mental and Nervous Diseases;* chapter 14 on *Motor Autonomisms and Reflex Actions*, in the *Psychological Bulletin;* chapter 15, *The Tendon Reflexes*, in this JOURNAL. In passing over the first two of these chapters, it may be relevant to ask the author's grounds for saying that "Descartes rejected Harvey's conclusions regarding the circulation of the blood" (p. 29), in view of his own statement that Harvey's discovery was understood by those "whom the authority of the ancients had not entirely blinded." (B. Rand, *The Classical Psychologists*, 1912, p. 171). The localization of function in the nervous system is traced down through the 17th century from Thomas Willis (1621–1675), who elaborated the principles of mechanical action as stated by Descartes and assigned to the cerebellum the rather independent autonomic and reflex functions (including the contributions of Robert Boyle, Perrault, and others who demonstrated the persistence of various vital functions after the destruction of the brain). Boerhaave indicated autonomic activities ('habits') "which taking their first action from the command of the will, do nevertheless afterwards continue to operate in a manner as if it was spontaneously," and "of which by continued use we are insensible" [by no means the first recognition of such acts], thus enlarging the range of recognized 'mechanical' activities. To these the 18th century researches and speculations by Astruc of Montpelier, the first to use the term 'reflex' in 1736, Albrecht von Haller, Robert Whytt, who particularly demonstrated the reflex functions of the spinal cord, Hartley, La Mettrie, Unzer, and others added details as to specificity of functions and carried the mechanistic movement to the point of inviting critical reactions against it.

Other bodily functions and reactions come in for consideration. Samuel Farr (1741–1795) suggested that, although as a "wise provision of nature" many activities did not require conscious guidance, the soul nevertheless was governor of the entire machine. The two Alexander Monros found that various actions of different levels of complexity could not all be accounted for "on the yet known principles of mechanism." Then come the discoveries of Sir Charles Bell and Magendie, as to specific functions of the posterior (sensory) and the anterior (motor) spinal columns. As to claims of priority the author quotes statements which are, in agreement with Boring's view (*A History of Experimental Psychology*, 1930, p. 36), in favor of Bell whose general conclusion, says Boring, was arrived at in 1807. It is to be noted that Fulton, a physiologist, takes the opposite position in his *Source Book of Physiology*, 1931. "Though one hates to pass judgment upon claims to priority," he says, "one cannot doubt that Bell was dishonest and that the chief credit of elucidating the function of the posterior as well as the anterior roots belongs to Magendie." It is unfortunate that this difference in opinion still exists. In considering the contribution of Johannes Müller and of Marshall Hall, on the elements of the reflex arc, Fearing characterizes Müller as dogmatic regarding "the mechanical aspects of the reflex act" and says that Müller "seems to have *recognized the possibility of psychical activity*

as a modifying factor in reflex action" (p. 145, italics mine). Modern writers are surely not forced to choose between alternatives like these in the reconciliation of historical contributions. A similar openness by the author to mystic factors as one of two alternatives is shown in his treatment of the controversy between Pflüger and Lotze. Nowhere can the reader find a clear definition of either of these factors or how it would operate in different degrees of uniformity of activity. Obviously some sort of clarifying criteria should be offered before the author could refer seriously to, or consider the "effect of concomitant psychical processes on reflex action," . . . "the opinion that psychic forces might be set free in any part of the brain or spinal cord" (p. 165). The reader wonders how far he can rely on what purports to be an impartial consideration of the various sorts of historical and controversial data presented.

In Chapter 12, *The Inhibition of Reflex Action*, the author recognizes the problem concerning 'psychic forces' as essentially one of the "modifiability of of reflex action," seemingly because it has become in the latter part of the 19th and the first of the 20th century so complex that no simple alternative can be presented; but one may well object when Fearing asserts that as against the crystallizing of problems of variability in psychology "around the concept of inhibition," these problems relate themselves in psychology 'to phenomena of attention and the effect of cortical or 'voluntary' processes upon concomitant reflex or involuntary activities" (p. 187). The situation is not exactly thus in psychology; the alternative to 'mechanical' reflex is not still that of a baffling dualism. Fearing generally speaks of "the effect" (singular) when he refers to supposedly 'psychic,' 'voluntary,' and other mystic and ill-defined factors. These factors seem to him somehow to be unitary, and possibly arbitrary. To the reviewer it seems somewhat questionable also to refer to inhibition in general as *negative response* (p. 188), except from a particular standpoint to which an historical writer can hardly be limited. What are negative and what are positive reactions from the standpoint of the data on such theories of inhibition as the 'center' theory of Setschenow, the 'interference,' the 'metabolism,' the 'refractory phase,' the 'drainage,' and the 'chemical' theory traced in Chapter 12? The unfortunate statement regarding the refractory phase effect "that the subsequent *stimuli* find the nervous conductor in a state of lowered excitability (refractory phase) as a result of the preceding impulse and hence fail to 'get through' " (p. 198, italics mine), will not clarify the situation to the uninitiated.

Following chapters 13, 14, and 15 (previously printed in journals), there is a final chapter on modern concepts. It contains a brief sketch of the expansion of the mechanistic physiology through Jacque Loeb, Bethe, von Uexküll, Pavlov, and others; and some indications are mentioned which, to the author, point to its decline. Fearing finds that in modern experimental work, the term 'reflex' is undergoing a diversity of meanings and concepts and he classifies these conceptions of avenues of approach into rather similar classes, both for physiology and psychology, such as the mechanical approach, the specific approach, etc.

Some general criticisms that one might make of the book are that references could have been put in the form of dates, so that the chronological perspective would at once be clear from the text without going to the bibliography at the end of the book; that foreign quotations be uniformly translated, etc. The reviewer's experience with American psychologists and graduate students in review work would indicate the desirability of translating at least all unambiguous

statements quoted from literature in foreign languages. There seems to be no advantage in such cases in maintaining the original untranslated statements of the authors. The style of the book is somewhat monotonous, and the author's reactions are rather uncritical. Occasional obscurities and at least one misspelling ('accomodation,' p. 82) occur. J. P.

Selected Readings in the History of Physiology. By JOHN FARQUHAR FULTON. Baltimore, Charles C. Thomas, 1930. Pp. 317.

In this skillfully organized volume Fulton, formerly Fellow of Magdalen College, Oxford, and now Sterling Professor of Physiology in Yale University, has brought together a collection of readings intended to introduce the student of physiology at first hand to some of the great names and great experiments in the past and present of physiology. The volume is illustrated with well selected portraits and drawings. The whole collection is of real interest to any scientist of catholic taste, but as this review is intended mainly for psychologists it will limit itself for the most part to an evaluation of those portions of the work whose contents are important for the student of physiological psychology.

In the first chapter certain of Lawrence J. Henderson's views on the fitness of the environment are reprinted. The conceptions of this writer must always be of interest to the theoretical psychologist. Passing over chapters 2, 3, and 4 on circulation, the capillaries, and respiration, we must note in the fifth chapter on digestion the excerpts from Pavlov's *The Work of the Digestive Glands* and from Cannon's 1898 paper, *The Movements of the Stomach Studied by Means of the Röntgen Rays*, which are, at least by inference, important.

The two chapters of the work previous to the final chapter are in the main significant to the psychologists, as they deal respectively with "Muscle and Peripheral Nerves" and "The Central Nervous System."

In the first selection from Galen's *De motu musculorum*, Fulton has chosen a passage which shows that this writer had already come to the conclusion that muscles can perform work only when they contract. The next selection is from William Croone (1633-1684), founder of the Croonian lectureship. In this selection Croone who, as the editor says, "despaired of ever knowing how the soul acted upon the body" held, admittedly as a speculation, however, that a liquid passing from the nerves into the muscles leads to their contraction. Certain of Willis's views on muscular contraction and Francis Glisson's proof that the volume occupied by a muscle does not increase during contraction are presented. The views of the Scottish physiologist, Alexander Stuart, upon muscular motion, which are included, show that even in 1739 the view of reflex action, earlier proposed theoretically, had not yet been accepted in physiology. Stuart believed that when he forced a probe into the *medulla spinalis* the resulting motion of the limbs was due to the fact that fluid in the cord was forced out through the nerves to the muscles, thus causing contraction. He further asserts that "voluntary muscular motion in a living animal is begun in the same manner, by an impulse of the mind or will on the animal spirits through the nerves, into the muscles." A selection is next given showing Galvani's fundamental experiment in electrophysiology. The remaining selections in this chapter are from the works which may be considered contemporaneous. These excerpts include Bowditch on the

all-or-none principle of contraction of cardiac muscle, Fletcher and Hopkins on lactic acid in muscular activity, Lapicque on chronaxie, Forbes and Gregg on action currents of nerves which are brought into activity in reflex response, Pratt on the all-or-none law in muscle, Hill and Hartree on heat production in muscle, Kato on decrementless conduction based upon studies in the narcotized nerve, Erlanger and Gasser on the analysis of action currents in peripheral nerve, and Adrian and Zotterman on adaptation and the results of stimulation of a single receptor by a natural stimulus.

In the chapter on the central nervous system, the first section, after the able introduction by the editor, is a translation from Descartes on his views of reflex action and reciprocal innervation. The Edinburgh neurologist, Robert Whytt, is represented by an excellently chosen excerpt which shows his conception of reflex action. The next selection is from Prochaska, who, as the editor says, "occupies an important position in the history of psychology for his introduction of the conception of a 'sensorium communi'—that region of the central nervous system which 'reflects' to the motor nerves the sensory impressions received by the brain." Alexander Walker, from whose excellent translation the selection by Prochaska is taken, occupies a strange position in the history of the development of the knowledge of reflex action. This is deftly indicated by the editor in a few introductory sentences and in a short quotation which shows Walker's strangely true and yet strangely false view in regard to the anatomical basis of reflex action.

In introducing an unusually long selection from Sir Charles Bell's *Idea of a New Anatomy of the Brain*, Fulton states the opinion upon the Bell-Magendie controversy that, "Though one hesitates to pass judgment upon claims to priority, one cannot doubt that Bell was dishonest and that the chief credit of elucidating the function of the posterior as well as the anterior roots belongs to Magendie." From Magendie himself is a well selected and important quotation which also bears upon the question of the function of the roots of the spinal nerves. Herbert Mayo, whose work, in the opinion of the editor, marks a true advance in the history of reflex action, is also represented by two significant paragraphs. Flourens is quoted on the physiology of the cerebellum, and Marshall Hall's most important contribution on reflex motions that are "independent of sensation and volition" is next chosen. After this follow selections from Ernst Heinrich and Eduard Friedrich Weber on vagus inhibition of the heart, Brown-Sequard on the functions of the spinal cord, Ferrier on the localization of cerebral function, Sherrington on correlation of reflexes and the principle of the common path, the proprioceptive system, and myotatic reflexes, from Harvey Cushing on the human motor cortex, and from Rudolph Magnus on animal posture. The last chapter of the book deals with certain miscellaneous contributions to physiology.

The bringing together of a work of this sort is a notable service. Beginning students too often lose sight of the deep roots of present knowledge. It seems hardly open to question that there is no way in which the student of any science can adequately comprehend the full significance of present conceptions if he is ignorant of the process by means of which these conceptions evolved to their present form.

Brown University LEONARD CARMICHAEL

Psychophysiology, II. Sensation. By LEONARD T. TROLAND. New York, D. Van Nostrand Co., 1930. Pp. xvii, 397.

This is the second volume of the author's projected tetralogy on the facts and theories of *Psychophysiology*, the 'science which endeavors to ascertain the laws that link consciousness or immediate experience with the variables and phases of (neural) response.' The first volume, it will be recalled, dealt with the general problems of psychology, and included a treatment of perception (the relationship between consciousness and the objective world). The second volume treats of the problems of sensation (the indirect relationship between experience and the afferent arc, beginning with the sense-organ stimulus and ending at the sensory projection areas of the cortex). The author has provided skillfully for the overlapping of these two fields by adding to the later volume an appendix of 'notes' in which are reproduced necessary sections of the first volume—a procedure which enhances materially the value of the book.

Although "Psychophysiology" has been selected as a group title for the entire series, intimate acquaintance with the contents of the present volume will convince the reader that the title is in this case a misnomer, and that 'psycho-biophysics' would have been a much more accurate description of the author's point of view. There is little of physiology in his treatment of the various departments of sensation, and, indeed, one feels that the psychological aspects are secondary; in the chapters on vision and audition especially, the approach is always from the side of biophysics, and the author's interest lies plainly in a correlation of physical stimulus and experience. This method of approach may be irritating to some readers, and they may find still further cause for dismay in the author's apparently ready and uncritical acceptance of results reported by Hecht, Fletcher, *et al.*

One may say without hesitation that Troland's volume, regarded as a whole, is an accurate digest of most of the more important experimental facts and theories in the realm of sensation, taken from the point of view just described. Troland makes an attempt to arrange the facts of the several departments of sensation under the categorical headings, Component Sensation, Composite Sensation, and Temporally Conditioned Sensation. In this way he obtains a parallel structure which is followed with profit through the various sensory departments; it is never rigid, and, when necessary, it is sacrificed for the continuity of the treatment. Troland's style is labored only at rare intervals, but he proceeds from point to point with a speed and a presupposition of background-knowledge which may well leave the untrained reader breathless. The book is patently intended for his colleagues and for their more advanced students, and by them it will be received as an important addition to their libraries.

The opening chapter (XIII) deals with 'The Concept of Sensation.' Its first section serves to define the term after a brief historical survey; this is followed by a section on the subdivisions of the afferent arc. A succeeding section follows Nernst, Lillie, Lapicque, Lucas, Adrian, and others in formulating a theory of permeability and polarization of nerve in terms of which the author can explain the more important phenomena of nerve action.

Chapter XIV undertakes a survey of the experimental and theoretical investigations of visual sensitivity. One is shocked to read a treatment of the 'psychophysiology' of vision which contains a dozen references to Hecht, but none to G. E. Müller and Ferree, and only very casual references to Hering,

Helmholtz, and Von Kries. Does this mean that the work of these investigators is to be included among the scientific 'duds' which the author warns us he will omit? Following the general plan of arrangement described earlier, Troland treats the stimulus, the refractive, and the neurological relationships of brilliance and of chroma (this latter term being used as a synonym for chromatic experience). The chapter concludes with a section on binocular sensory relationships. It is eclectic in point of view, and the author presents the more important visual theories, although it is apparent that he inclines toward a modification of Hecht's general theory, together with a color-theory which adapts the Helmholtzian interpretation to modern knowledge of nerve action. Troland's discussion of the classical visual theories is brief to the point of curtness, and it is perhaps this brevity which makes his style labored at times in comparison with the lucidity of the preceding chapter.

The chapter on auditory sensation also is written from the biophysical point of view, and it follows his general plan of classification. It will be a singularly disturbing piece of reading to those who find Fletcher's methods inadequate, for Fletcher it is who serves as the guiding genius of this chapter. On the theoretical side, Troland leans towards a pressure-pattern theory in which pitch is represented by impulse-frequency along the nerve path and intensity by total number of pulses per second whether due to number of paths or frequency of impulse.

Succeeding chapters deal with facts and theories of taste, smell, cutaneous, kinesthetic and organic sensitivity, and are more nearly 'psychophysiological' in flavor. The author shows no acquaintance with the negative results of the more recent investigations of Von Frey's correlation of cutaneous experience with receptor and he presents this correlation without adverse comment. For some reason, he makes no reference at all to Boring's classic investigations of internal sensitivity in his chapter on organic sensation, and neither of his volumes affords an adequate presentation of the better-known facts of composite tactual sensation.

A more detailed review might list additional sins of omission and commission and might include a criticism of Troland's translation of various German terms; but such a review could not detract from the importance of the contribution Troland has made in this volume. As a concise, accurate and well-documented textbook of the biophysical background of experimental psychology, it leads one to anticipate with high hope the appearance of the author's succeeding volume on cerebration and action.

Cornell University JOHN G. JENKINS

Human History. By G. ELLIOT SMITH. New York, W. W. Norton & Co., 1929. Pp. xviii, 472.

The book relates man to his primate ancestors, describes the fossil remains of early specimens of *Homo*, recounts the primitive industries, distinguishes the great racial migrations, describes the dawn of human civilization in Egypt, and traces the main stages of development in Asia, Crete, and Greece, telling the story of how "mankind acquired culture and with it social unrest, dangerous practices, and methods of cruelty."

The author approaches human history as an anatomist. He writes "in a scientific laboratory where the principal device for the study of mankind is the process of dissection." But since the "true study of mankind" must look to

"the thoughts and feelings that provide the motives for men's behavior," the history is more than an account of the body and of its organic functions. It is, in part if not in essence, psychological.

Smith's theory of human derivation, with its emphasis laid upon cortical expansion and efficiency of the receptors, has been elaborated in earlier works. Except for the author's insistence upon certain early Tarsioids in the ancestry, the theory is commonly held by anthropologists. Little is added to earlier writings in the present general exposition, where the author's main concern is to develop the doctrine that man endured, with but little modification, for hundreds-of-thousands (possibly millions) of years "decent, generous, and peaceful" and wanting any of the "customs, beliefs, arts and crafts, social and political organization of civilization." For this view and for the materials supporting it, Dr. Smith makes a heavy acknowledgment to W. J. Perry, the author of *The Children of the Sun* and other studies of early civilizations. It cannot be said that the anatomical method has here added much to our lean information upon the actual behavior and accomplishments of early man. Smith depends (as other anthropologists have) upon tools and other artifacts and upon the want of evidence of social living. He finds primitive man but slightly inventive and explains the wide spread of common implements and other cultural products as due to diffusion throughout the earth. He has no working scheme of the psychological functions of primitive man to lead him toward a plausible depiction of this primate's daily life. Instead he makes use of the assumption that man's final approach to a civilized state arose from his "endeavors to understand the constitution of his own body and to preserve the life that animated it." This studious lust in man, fed by an instinct for self-preservation, suggests a sophistication among the Egyptians of about 4000 B.C., for which the dull and peaceful ploddings of preceding millenia do not seem to prepare either primitive man or the reader of *Human History*.

The *History* contains a great deal of substantial and orderly knowledge about early man. It may well be read by the layman along with E. A. Hooton's *Up from the Ape* (1931), an entertaining and dependable account, written in terms of physical anthropology, of primate history and of the place therein of the various species of Man.

The new arguments adduced for the Golden Age of pre-civilization must be left to the ethnologist and the psychologist. It is a striking fact that the family (the only really primate and primitive form of organization allowed by Elliot Smith) should be made the locus of primaeval 'decency, generosity and peace' just at the time that it is being made, as a part of the battle of the sexes, the main seat and occasion of disorder, maladjustment, and psychotic disease.

Cornell University MADISON BENTLEY

L'hallucination: Études cliniques. By PIERRE QUERCY. Paris, Félix Alcan, 1930. Pp. 559.

This large volume, which is crammed with references to old and recent works dealing in any way with the subject, *e.g.* Descartes, Spinoza, Baudelaire, Müller, Sidis, Jaensch, Urbantschitsch, Rouhier, Rothschild, and Wheeler and Cutsforth, could have been condensed into about half the present compass with profit to

the author's thesis. The thesis is that genuine hallucinations exist, meaning by hallucination a full sensory experience in the absence of any external object to arouse it. His proof (aside from affirmations of hallucinated persons—from common dreamers to St. Theresa) consists in trying to show, in separate chapters, that illusions of proof-reading, synesthesis, sensitivity in an amputated limb, after-images, eidetic images, virtual images, dream experiences, alcoholic hallucinations, peyotl experiences, and the classical auditory hallucinations are all links in a chain, which, in spite of analysis, in spite of large concessions made by those who claim to be hallucinated, has in every link an irreducible sensory content even without any object to arouse a sensation. This sensory content "without object," being an integral part of all perception, makes hallucinations out of all perceptions. Quercy thus turns the tables on those who seek to outlaw hallucinations as mere images, beliefs, projections, or actions "as if hallucinated."

How is an hallucination possible? His answer is "in the absence of the object, the movement which has made an image out of the perception can reproduce itself in the inverse direction, and resuscitate the perception, identical with what it has been under the shock of the object, with what it would be if the object were present" (p. 12). Another correlative explanation is based on the theory of primary and secondary qualities, or elements. The *primary* sensory elements demand the presence and the action of the object, while the secondary sensory elements, "constitute the essence of *bona fide* perception," and "are already hallucinatory, if they detach themselves from the primary sensory core." They constitute "the classical hallucination." "Detached by sickness or by a dream from their normal, primitive, sensory element, any secondary group might associate itself with any primitive group, sometimes with unconscious groups; a lesion of the pharynx, of the ear, of the eye, of the viscera, any of these could give the unknown primitive sensory core of an hallucination. As it associates itself with another primary core, with a convenient and adequate primary group, and without having changed, it would be a legitimate perception" (pp. 35-36). "Our perceptions of red, of the third dimension, of ambiguous perspectives, and of the insults of a non-existent persecutor all possess, in spite of the growing charge of interpretation, the same rights to be called perceptions, sensations, *esthésias*, and the same rights to appear as such to us when we undergo them" (p. 494).

As seen above, although he bases his argument upon them, the author misinterprets and misapplies the processes involved in the perception of color, of ambiguous figures (*e.g.* cubes), and virtual images. Contrary to his contention, there *is* something objective, and something different for different colors, when one sees red or green; there *is* a perfectly clear physical explanation, as well as a real physical cause, when one sees a virtual image in a mirror; and far from the ambiguous cube's giving "more than it has, being this incomplete and poor object" (p. 510), it is richer by just two lines than the same sort of skeleton drawing of a *bona fide* cube.

As one might expect, the line the author has taken leads close to, or into, obscurantism. "The visionary claims to see and to hear sights and sounds as legitimately as we see tonight the multi-colored and changing after-image of a flame. . . . To what degree of absence, and why to this degree of least presence ought one to say that the object is too much absent to be perceived? When is

the perception too indirect to be possible? When is the perception that has no immediately present object so unusual that one can contest it and cry to the subject: 'You have only the behavior of perception'? When on the contrary do the perceptions without present objects, if their victims persuade us, become sufficiently veridical—(esthésique)—and consequently formidable, in dreams and hallucinations, to counterbalance the ordinary perception and to render the universe doubtful?" (Pp. 26-27.)

This book is mainly a philosophical treatment of the subject. Its value for psychologists lies in its mass of references to and quotations from others' observations, in its reports of experiments on eidetic imagery (negative results with 200 children), and in its details of self-observations during alcoholic hallucinations, peyotl intoxication, and self-study of after-images. A good many clinical histories of hallucinations are also given. There is no index.

Louisiana State University PAUL CAMPBELL YOUNG

Nouveau traité de psychologie, Vol. 1. By GEORGE DUMAS. With the collaboration of C. Champy, A. Lalande, L. Lapicque, R. Perrier, P. Rivet, A. Tourney, H. Wallon. Paris, Félix Alcan, 1930. Pp. 425.

The nine-volume revision and enlargement of the two-volume *Traité de Psychologie*, edited by Dumas and published in 1923 and 1924, promises to be a valuable source book. The volumes are to cover most of the major fields of contemporary psychology. The present (first) volume comprises several chapters of introductory material from allied sciences and two chapters dealing with the general problems and methods of psychology. The following six volumes are designed to present the main facts of normal psychology, while the final two will be devoted to the approach to the science through the medium of the pathological. Since a good deal of traditional French psychology has been biased in the direction of the philosophical disciplines, it is interesting to note that the present work is ruggedly empirical in its approach. The general point of view as stated in the introductory chapter, written by Dumas himself, is catholic throughout, with the emphasis rather upon the statement of ascertained fact than upon the discussion of theory. It is evident too that the editor is opposed to the restriction of the subject matter of psychology to those facts which fall within the limits of what has been conventionally called experimental psychology. Psychology for him is "a science in which introspection plays an essential preliminary rôle in the description of facts and the analysis of mental mechanisms, in which biology and sociology study the biological roots and the social evolution of the mental functions, and in which pathological psychology, psychopathology and neuropathology make their invaluable contribution through the analysis of sensory-motor and mental disturbances and the cerebro-organic conditions which underly them." In keeping with this point of view, fully one-fourth of the work is to deal with the biological bases of psychological phenomena. In general one gathers that the order of the topics will follow approximately that of the older *Traité*.

Of the eight chapters in Volume I six are non-psychological. The titles of these are: The Place of Man in the Animal Series, The Data of Anthropology, The Physiology of Age and Sex, The General Physiology of the Nervous System,

The Special Physiology of the Nervous System, and The Biological Problem of Consciousness. Of these, the chapters on anthropology and the physiology of age and sex are new, while the others, although present in the older work, have been either revised or entirely rewritten. The various articles are without exception clearly written and well illustrated. Of special interest to an increasing number of American psychologists is an excellent summary by Lapicque of most of the important work on chronaxy.

Cornell University R. B. MacLeod

L'hallucination: Philosophes et mystiques. By Pierre Quercy. Paris, Félix Alcan, 1930. Pp. xxvii, 381.

In his introduction Quercy contends that a number of speculative philosopher from Plato to Bergson have been considerably more concerned with hallucinations than with perceptions or images. The treatise proper is divided into two parts, the first a review of the writings of Spinoza, Leibniz, Taine, and Bergson, pertinent to imagery, hallucination, and perception; and the second, a psychological interpretation of the hallucinatory experiences of Sainte Thérèse. The hallucination according to philosophers and mystics is thereby contrasted with an appended note as to the view of a theologian (Gerson).

Part two, an attempt to clarify our concept of the hallucination by reference to a life full of them, started with the citation of a dispute as to whether Sainte Thérèse owed her fame to her neuropathological state, or whether her occasional lapses had nothing to do with her visions and writings. The general conclusion to a discussion of her symptoms of hysteria, sexual frigidity, and other alleged psychopathological conditions seemed to be that she was but slightly if any different from what might be expected of a woman with her interests and ability.

While the first part of *L'hallucination* summarized well the contributions of certain philsosphers to our understanding of the word, part two impressed the reviewer as being fruitless. It does not appear reasonable that any psychological phenomenon can be made clearer by reference to its manifestations in a life that ended almost 400 years ago. Likewise, the practice of establishing the reality of an individual's experience by reference to his own writings is hardly to be commended. Quercy's book is interesting chiefly as an historical approach to what has been at best a cloudlike concept.

DePauw University Stephen M. Corey

Brain and Mind. By R. J. A. Berry. New York, The Macmillan Co., 1928. Pp. 608.

The major portion of the book is concerned with (1) the phylogenetic development of the human nervous system considered first as a whole and then later with reference to its well defined divisions; (2) the gross and minute anatomy of the major portions of the human brain; and (3) the available knowledge as to the functions of these portions. A systematic presentation of the materials is accompanied by 131 figures. The treatment of the autonomic nervous system both in the text and in the illustrations is exceptionally clear. The exposition of the phylogenetic and anatomical material is simple, direct, and admirably suited for beginning students in neurology.

The minor portion of the book is avowedly psychological in nature, presenting two concepts in particular; (1) the familiar thesis that the neuron is the unit of mind, and (2) a technique for mental examinations in which physical and mental measurements are combined. The psychologist will find little new in the arguments supporting the first concept. These arguments would support no theory that any mental disorder exists for which there is not a cerebral insufficiency of some sort.

The clinical psychologist would be skeptical of the relationship assumed to exist between the anthropometric scores and mental ability. A review of the clinical cases given in a concluding chapter would appear to show that when the physical measurements support the mental tests (Binet and Porteus) they are pertinent; when they do not, they are meaningless.

Excellent percentile tables for ages 7-19 yr. and for adults are given for the following measurements: brain capacity, standing stature, sitting stature, bodily weight, grip, and vital capacity. Percentiles are given separately for the two sexes.

The references cited are lacking significantly in those of a psychological nature.

University of Utah DOROTHY NYSWANDER

Essentials of Psychology. By W. B. PILLSBURY. Third Edition. New York, The Macmillan Co., 1930. Pp. ix, 466.

In this second revision the greater part of the book has been rewritten, but the general outlines of content are preserved as far as compatible with recent developments in psychology. Psychology is now defined as "the science of behavior and of the knowing functions of man" (p. 2). This definition frankly avoids the controversies concerning mind and consciousness and recognizes that psychology now covers a broader field than mere conscious behavior. Pillsbury finds introspective as well as objective methods possible because one does not actually observe one's own act while it is going on, "but examines it a moment later in memory" (p. 3). The use of the test method in practical applications of psychology is now recognized. Modern work on cerebral functions, on the refractory phase, the all-or-none law, as well as that by the *Gestaltists*, etc., is briefly indicated, and some of the recent experimental results on the senses, the emotions, etc., are given. Helmholtz's resonance theory of hearing, though favored, is erroneously stated in regard to several details, and much of the best recent objective work by several authors is omitted. The old dimensions of the basilar membrane remain as well as the earlier view of Helmholtz, that the fibers vibrate individually, and objections to the theory on these erroneous data and views are stated as of old. The treatment of instincts has been revised and brought forward to constitute chapter 4, under the caption, The Native and Acquired Factors in Behavior. Here there is some admission of innately determined structures, but the specific developments in activity trends are due to environmental factors, except for the effects of 'selection' in the terminal movements, *i.e.* types of satisfaction or dissatisfaction which accept or reject. On this basis we still have collection, workmanship, fears, laughter, racial, parental, and social instincts, and sympathy ('suffering with others'). Instinctive acts

and acquired acts are only relative divisions; 'instinct' is limited to 'drive' and to selection; but learning, as referred to in this chapter and discussed in the next two chapters, is still the result of repetition of acts 'stamped in' by pleasure or native selectiveness. The book continues to emphasize results and theories rather than scientific methods of investigation, and references are only to books. A final brief chapter sketches the general outlines of the genesis of psychological 'schools.'

The author's clear, direct style is a real joy to readers, but the method of the book is rather to impart information than to arouse problems and stimulate investigations. The new edition has several merits as an elementary textbook.

J. P.

Handwörterbuch der psychischen Hygiene und der psychiatrischen Fürsorge. By O. Bumke, G. Kolb, H. Roemer, E. Kahn. Berlin, Walter de Gruyter & Co., 1931. Pp. vi, 400.

This work ought to prove to be a very serviceable handbook on mental hygiene, especially from the psychiatric point of view. It is alphabetically arranged and contains both a table of contents which conveniently lists the author of each article and a detailed index of authors cited and items. At the end of each article comprehensive bibliographies are appended which cover such subjects as alcohol and its effect, the hygiene of industry, marriage and eugenics, the care and therapeutics of various types of mental and nervous diseases, psychoanalytic treatments, the biology and psychology of criminals, the criminally insane, and a very able summary of the whole subject of psychotherapy. The questions of suicide and of a wholesome philosophy of life are conveniently and comprehensively treated. The bearing of spiritualism and mystic approaches on mental health are also given. There is helpful material on statistics, various laws concerning insanity and the traffic in opium together with a rational exposition of methods of training in nutrition, and clothing and its relation to a proper bodily development. These subjects are samples of many others which are likewise treated with a minimum of emphasis on theory and hypothesis and a maximum of experimental orientation. Numerous tables are available but outside of a few diagrams and graphic figures there are no illustrations. The reviewer hopes that this clearly written and conveniently arranged handbook will soon be translated for the use of English-reading psychologists and psychiatrists. It certainly should be included in the libraries of our psychopathic institutions and be individually available to social workers and practitioners in the field.

University of Iowa Christian A. Ruckmick

Culture and Progress. By Wilson D. Wallis. New York, McGraw-Hill Book Co., Inc., 1930. Pp. xii, 503.

This large volume is devoted to a critical survey of culture and progress from a broad historical approach and with a view to throwing light on contemporary society. Some of the leading problems considered are: culture areas and traits, their origin, nature of development and spread, theories of progress, and the criteria of progress. The author attacks with vigor and confidence many generalizations put forward by different 'schools' of older anthropologists,

several of whom are still important contributors to anthropology. The conception of 'culture areas,' as is well known, has spread widely of late not only in anthropology but also in sociology and biology. "While much of the aboriginal world can be divided into culture areas, there is little information with regard to some large ethnological areas." Several conclusions which are justified by present information are given on page 10 ff. We can indicate some of them only briefly: A culture is unique; but it does not travel *in toto* into other culture areas. It becomes modified, each culture influencing and becoming changed by other cultures with which it comes into conflict. One culture does not supplant another, but certain culture traits even flourish better in new than in their native soil. The traits comprising any culture are so interrelated that innovations affect the entire culture complex. Individuals participate to different extents in any present culture, and when "allowance is made for the more limited range of tribal culture the individualities of men in primitive life are perhaps as marked as those of civilized men" (p. 12). Illustrations, but no adequate statistical data, are given to support even this last statement; because, apparently, such data in "numerical results" are mistrusted (p. 45) as being often used to justify foregone conclusions. Sapir's principle of 'necessary connection' or 'necessary presupposition' as to priority of culture techniques and the principle used by certain archeologists in developing an historical perspective of the development of art; namely, that the extent of distribution of any culture is positively correlated with its age, are challenged. Often (*e.g.* in modern times) the reverse is what actually occurs. The view that culture is centrifugally diffused is questioned on the basis of history. In general, the historical reconstructions of cultural developments when based on 'principles' of priority of different cultural traits as inferred by ethnologists rather than from close knowledge of actual history is strongly condemned as the construction of mere 'gardens of Eden.'

This definition of progress is finally arrived at: "Progress consists in performing a function with ease or with greater proficiency or efficiency, and in doing something desirable" (p. 428). The measure of the degree of desirability is admitted to be an especially difficult one, and the author's suggested criteria are not free from subjectivity, but, at least, broad circumstances must be considered in evaluating desirability. When Wallis argues for the uniqueness of the culture of any civilization or of all historical induction as merely the summary of what has taken place—as opposed to considering different changes as due to 'inexorable laws of nature'—he presents an argument that can as well be applied to the development of any *individual;* and he seems to be regarding mind, in the development of cultures, as an arbitrary factor. Certainly, *some* generalizations are helpful, even though they do not seem to apply universally in our modern world. Several such generalizations on progress, as Wallis indicates, have evidently been based on insufficient and old data, and have disregarded concurrences of numerous elements of progress in our period. 'Uniqueness,' however, is a conception that needs careful examination. The uniqueness of a civilization, say of Greek civilization, certainly does not make the entire complex so unrelated to all other events as to make it non-predictable. Uniqueness is not a scientific conception. The question is always one of *degree* of predictability. Uniqueness does not imply total discreteness; it should probably not be used in an absolute sense. The author's collection of data on the details of the phenomena

of progress is valuable, but one wonders what degree of reliability and of selectiveness is possessed by his numerous citations and quotations. Certainly a number of the references, well-known to the reviewer, could not stand without question in this regard.

J. P.

Abstracts of Theses, The University of Chicago: Humanistic Series, Vol. 7. Chicago, The University of Chicago Press, 1930. Pp. xiii, 712.

This volume includes the abstracts of theses submitted in the year September 1928, to June 1929, in the Graduate School of Arts and Literature, the Graduate Divinity School, the Graduate School of Commerce and Administration, and the Graduate School of Social Service Administration. The departments in these schools which are probably of most interest to psychologists are represented by abstracts as follows: philosophy, 2; psychology, 10; education, 17; economics, 4; political science, 2; history, 14; sociology, 6; anthropology, 4. In all other departments there are 50. There are thus in this volume alone, disregarding those in the *Science Series, Vol. 7*, for the same period, abstracts of 109 Ph.D. theses,—quite an interesting and illuminating condensation of researches in many lines of work bearing directly on human behavior. Six of the psychology theses have already been published—either in full or in abridged form—in well-known periodicals.

As to the others, one, by C. W. Brown, found on rather small groups of graduate and college students (1) that retention of paired nouns is a function of their (estimated) frequency of occurrence in past experience; (2) that the retention of any given words arranged in meaningful relation with other words is a function of possible words conforming to this relation and varies inversely with the numbers of these possible words; and that (3) conceptual systems markedly strengthen the retention of elements in the different classes of objects, even against the influence of other, older principles of learning. Marion Monroe found, from 1612 drawings produced under controlled conditions by 134 children (ages 2 to 5 years), a correlation of 0.66 between assigned drawing scores and chronological age, and of 0.68 between the drawing scores and Stanford-Binet mental age. When mental age was held constant statistically in the first case and age in the second, the correlations were 0.24 and 0.31, respectively. Color preferences of the children were also obtained by different rather natural means in their play. Yellow was consistently preferred by the 2-yr.-old children and red by the 3- and 4-yr.-olds. Yellow decreased and red increased in preference with age; green and blue likewise increased, the green more consistently. Brightness and saturation differences were, however, also effective factors in influencing preferences. The order in which the names were assigned correctly to the colors used was blue, red, green, yellow. The difference between the first two of these was slight. Loh Seng Tsai found the gradual withdrawal of guidance in maze learning by rats to have no advantage over abrupt withdrawal. Several different methods of attack on the problem were used and blind-alleys were closed for 'guidance.' Georgiana Elnora Wylie found on 162 college students, by a variety of procedures, that the learning of "a series of twenty-four lists of items" was more effective by the whole than by the part method when both methods were

equally practiced. No reliable differences in retention were obtained. Practice effects, differences in ability in the sort of memory involved, etc., were controlled.

Data for accurate statistical comparisons are not available in these abstracts but are probably obtainable by anyone desiring them. Several abstracts in other departments deal with aspects of psychology. The entire volume is well worth going over with care by readers interested in psychology. A new volume is to appear every year.

J. P.

Types of Men: The Psychology and Ethics of Personality. By EDUARD SPRANGER. Translated by Paul J. W. Pigors. Halle (Saale), Max Niemeyer, 1928. Pp. xii, 402.

This book lies in the borderland between systematic psychology and ethics. Spranger is best known for his theory of personality types, but the present volume deals with this problem and the application of his categories of personality to problems of ethics and history.

The book is divided into four main sections. The first deals with the philosophical basis. For Spranger psychology must be more than a study of physiological reflexes. The individual can only be understood in terms of meanings and values which rest in a configuration of culture and universal psychic elements. His primary interest is to work out the more general forms or types of personality. Historical backgrounds are for his purpose incidental. As he puts it, "The constant complication of historical relations obstructs our vision of the fundamental pure structures."

The second section deals with these ideal basic personality types or "attitudes." There are six of these: the theoretic, the economic, the aesthetic, the social, the political, and the religious.

The pure theorist is an intellectualistic egoist. He is coldly objective, logical, thoroughly scientific. He tends to escape the world of facts in his interest in the purely conceptual structures which he builds. The economic type, on the other hand, is controlled by utilitarian values, by an intense desire for self-preservation. Work, steadiness and the virtues of material success give the key to his values.

In the aesthetic type art furnishes the fundamental "meaning-direction" in life. Feelings, sensations, and concrete perceptual experience constitute the foundation of his values. "The aesthetic formation of existence is only possible if one loves the human, all too human, if one also loves the 'veil of Maya' and finds in one's passions, joys." Form and beauty constitute his basic values.

The social type is dominated by "the value affirming interest in another being and the taking-the-place-of-another" or sympathy. Love of others rather than love of self is the fundamental motivation. In contrast stands the political type which is characterized by the craving for power, for egocentric control of others. "Power is thus the capacity and (usually) the will, to posit one's own value direction in the other, either as a permanent or a transitory motive." Leadership, however, demands self-control if it is to be most effective.

For Spranger, however, the religious type has the greatest merit. This attitude finds its deepest meaning in the totality of life. "God is the objective principle which is thought of as the object of the highest personal value exper-

ience." And "a religious man is he whose mental structure is permanently directed to the creation of the highest and absolutely satisfying value experience." Morality then becomes the "harmony of the individual with the highest world-meaning which he is capable of grasping." This means, of course, that the religious type goes beyond perceptual experience and, moreover, must infuse his conceptual world with divine meaning, for "anyone who can no longer call a God his own gives himself over to the devil."

The third part of the volume treats the consequences of these six-fold types for ethics. There are many one-sided systems of ethics which must give way to a morality in which the collective duties are correlated with personal or individual aims in a hierarchy of values in which the religious attitude stands supreme. The latter rests upon the highest development of personal idealism in which all other attitudes are subordinate to the absolute standard that transcends time and circumstance.

At the outset of the final section the author points out various complexities which arise in actual life. These ideal types never appear in pure form. In treating actuality he is led naturally to discuss the relation of history and culture to the individual. The individual is not merely the product of his cultural *milieu* but possesses also a strain of originality and uniqueness which rests upon the very nature of his "soul" structure. Thus understanding and participating in the historical process does not rest on mere imitation but on a combination of historical factors with the self-determined unique interpretation. "In the result we approach the conception that mind as a great mutual medium includes the individual and the objectivations which grew out of the summation, reciprocal effects and stratification of innumerable individuals."

The last chapter presents briefly a sketch of biographical analysis as one possible method of understanding personality, not merely a statistical enumeration of traits but a sympathetic insight into the inner life and meaning-values of historical personalities. In conclusion Spranger re-states his belief that at all points life is made up of meanings in which the "totality of the mind exists in every mental act" but with varying degrees of subordination of other meaning-directions to the "dominant content" at the moment. And the highest life will always find this "dominant" feature in the "divine" soul-structure expressed in the religious attitude.

It is difficult to evaluate so stimulating a book within the limits of a review. For the American psychologist interested in the practical analysis of personality traits the volume may seem a strangely philosophic and religious rather than a "strictly" psychological presentation. But for those of us who are interested in the underlying conceptual formulations about personality, the book is highly provocative and is bound, in time, to influence not only our ethical interpretations but our psychological investigations as well.

University of Wisconsin KIMBALL YOUNG

Mental Radio. By UPTON SINCLAIR. Introduction by William McDougall. Pasadena, U. Sinclair, 1930. Pp. ix, 239.

This book seems to call for notice among psychological reviews both because it is about things "mental" and because it is introduced and endorsed by Professor William McDougall, who is, according to the author, "probably the 'dean' of

American Psychologists" (p. 205). The introduction vouches for the author as "an able and sincere man with a strong sense of right and wrong" and it suggests that Mrs. Sinclair, the receiving station of the book, is "one of the rare persons who have telepathic power in a marked degree and perhaps other supernormal powers" (vii). Since the 'dean' hints that the Sinclair evidence for telepathy and other unusual powers is "conclusive," it had better be left at that in the present review. May we, however, promise the prospective reader interesting glimpses of the life of the Sinclairs while Upton "was busy trying to reform America," of Jan the Psychic, who "astonished physicians of Southern California" (p. 27), and of many intimate happenings in and around Long Beach, Pasadena and Los Angeles? We may at least assure him that he will discover what Mr. Sinclair and his sponsor dean understand by a scientific experiment. He will soon observe for himself that the simple logic supporting the conviction of the author is that his alleged successes or *hits* could not be due to chance or fraud and they must therefore be due to telepathy.

Among the most entertaining tales in which the psychic powers of the "witch-wife" come to light are those connected with the heroine's childhood in the "black belt" of the "far south." The eldest daughter of a Mississippi judge, planter, and bank president, little Mary Craig Kimbrough *got* the feelings of people by "intuition." She also intuitively knew when her mother wanted her, that Mr. B was waiting at the house, and that poor Jack London was in trouble. She could forecast, too, by dreams; and she was terrified by snakes, fire, and the fact of telepathy. In more mature years, she exchanges "by the laying on of hands" her own headaches for her husband's indigestions, and she learns how, by mere willing, to make the stubborn Jan "do whatever she pleases."

Anyone familiar with the writings of a half-century on Psychic Phenomena (usually initialed in caps!) will recognize the character of the evidence, most of it from the comparison of drawings mind-read from husband to wife. Sometimes the exact physical—not to say anatomical—conditions of the mental radio are nicely observed by wrapping the husband's sketch in green paper and placing it upon the "solar plexus" of the receiving station. Some of the author's interpretations of the telepathed drawings are worthy of comment; *e.g.* the snake that became a crushed kitten, the wild animal that suggested a chorus girl's legs, the caricature of a bald head which caused the wife to see "Upton's face," the hemorrhage which was clairvoyant for hemorrhoid, and the boy's foot which suffered "a strange telepathic transformation" into a lion, dog, or pig snout. Those alienists who discern (as they think) the type of a patient's deviation in his transformation of the Rorschach ink-blots would find food for thought in Sinclair's 'telepathed' figures. Since the author of *Mental Radio* ignores his failures and since he has no adequate method either for recognizing or accounting for his successes, it may be superfluous to remind him of Lord Bacon's shrewd remark that "men mark where they hit not where they miss." The hostile critic of *Mental Radio* will be disarmed by the foreknowledge that "those who throw out these results will not be scientists" (p. 176), and by the discovery that it was only "loyalty to the nature of the universe" (p. 220) that compelled the author to publish the book in spite of the sane advice of relatives and friends in Chapter XXV. M.B.

An Experimental Investigation of Social Behavior Patterns in Young Children. By ESTHER VAN CLEAVE BERNE. University of Iowa Studies: Studies in Child Welfare, Vol. IV, No. 3. Iowa City, The University Press, 1930. Pp. 93.

For all the present interest in social behavior, we must remember that careful factual and experimental analysis of this behavior is just beginning. Hence we should perhaps not be surprised to find a student setting out, as the author did, with the ambitious purpose of "describing and measuring the patterns of social behavior in children of several pre-school groups," as if there were a definite number of particular "patterns" which with a little intelligent industry could easily be found and measured. We find no attempt either to define "patterns of behavior," or to deal with the difficulties involved in isolating definite items of such behavior and working out reliable methods for their investigation.

Three methods of measuring social traits were used in this study: a rating scale, a series of experiments dealing with selected traits, and finally observational studies yielding "observational scores" of the various traits. Thirty traits believed to include all the social behavior patterns of pre-school children were arranged in a rating scale, with 5 degrees of each trait to be distinguished. The traits are indicated by such headings as (trait 6) "Coöperates; works or plays with others; works or plays jointly," and for trait 12: "Kind: is disposed to do good and confer happiness; is benevolent; well-disposed." Instructions to the raters were carefully designed to avoid the "halo effect," by requiring them to rate the entire group on one trait before taking up another; and "good" and "bad" ends of the scale were alternated to counteract a tendency to check at one end of the blank. Tables show the average ratings by 3 persons (sometimes 2) of 104 children. The average reliability of 3 persons in rating 30 children in common is $0.74 \pm .06$.

Tables also show correlations of the ratings with M.A. when C.A. is held constant, and *vice versa*. Numerous detailed conclusions are drawn (pp. 41 and 44) some of which do not seem to agree with the data. For example: "When the influence of mental age is ruled out, . . . in the Kuhlmann group, chronological age gives a reliable positive correlation with rivalry; a low positive correlation with ascendance; and a rather high positive correlation with socially controlled behavior." Yet the corresponding correlations in the tables are $-0.11 \pm .12$; $+0.01 \pm .12$; and $+0.44 \pm .10$. Clearly the tables are badly confused. The reader has no way of knowing what the results actually are, or whether they really furnish any ground for the conclusions given.

The tables as printed reveal a peculiar situation in that the relationships between ratings on the various traits and mental or chronological age are very different for the Stanford-Binet and for the Kuhlmann groups. For example, with chronological age constant there is in the Stanford group a negative correlation ($-0.57 \pm .08$) of mental age with "socially controlled behavior," while in the Kuhlmann group mental age shows a high positive correlation ($+0.70 \pm .06$) with the same trait. This sort of contradiction is not even mentioned as such by the author.

Some ingenious experiments were devised to measure some of the leading traits, such as obedience and respect for property rights. In general, the scores on the experiments for the various traits showed high correlations with the ratings. These experiments seem suggestive and well worth the attention of other investigators. Only in general do observational scores "parallel the ratings."

As a piece of pioneer work, this study is interesting and suggestive. Along with other pieces of work it helps in its way to provide a rough preliminary chart of the field of early social behavior. Now perhaps we are ready for the more analytical and more rigidly critical work which is needed.

Smith College — MARGARET WOOSTER CURTI

James H. Hyslop—X, His Book: A Cross Reference Record. Collated and annotated by GERTRUDE OGDEN TUBBY, formerly Secretary of the American Society for Psychical Research. Preface by Weston D. Bayley. York, Pa., The York Printing Co., 1929. Pp. 424.

Although few psychologists will be interested in this book, the author and Dr. Bayley (a neurologist of Philadelphia) think it is from the study of such material as it contains that "the only possible science of psychology must grow, as it inevitably will." Here is presented annotated transcripts of stenographic records of 29 séances attended in 1924-25, in New York, London, and Paris, by Miss Tubby for the purpose of providing opportunity for evidential communications of the cross-correspondence type from James H. Hyslop, deceased in 1920. Miss Tubby was trained in the natural sciences and spent 13 years in association with Hyslop. Although her conviction to belief in "communication" occurred as early as 1907, she says it could rest upon the evidence in this book alone. Bayley deplores "the status of contemporaneous psychology," since, "as related to its possible future, [it] bears the same general relation that alchemy once did to modern chemistry." His criterion for evaluating psychology seems to be its adequacy to explain the philosophical problem of the relation between the 'mind' and the 'body'.

Stanford University — J. E. COOVER

A Volume-Year Check List of Psychological and Allied Journals. By EDMUND S. CONKLIN. University of Oregon Publications, Vol. 1, No. 2. Eugene, Ore., University Press, 1931. Pp. 22.

This very useful little pamphlet contains, for 92 journals in psychology and some allied fields, the name of each journal, its proper abbreviation in bibliographical work, the names and addresses of its editor and publisher, a numerical list of the volumes published, and the date or dates for every volume. It should be a great aid to bibliographical precision for editors, for psychologists engaged in bibliographical work, and for those many authors with limited library facilities who would like to avoid the wrath of editors by making their citations of the literature exact and complete. How often one finds himself with all of a reference but the date! And, while no secondary source can be so reliable as the original volume itself, the chance of undetected typographical errors must be small in a list of consecutive volume numbers paralleled by a list of consecutive dates.

E. G. B.

The Mighty Medicine, Superstition and its Antidote: A New Liberal Education. By FRANKLIN HENRY GIDDINGS. New York, The Macmillan Co., 1929. Pp. 147.

The author diagnoses the ills of education, locates their source, and prescribes a remedy. His vigorous treatment displays a remarkable breadth of accurate knowledge, draws heavily upon the sciences of anthropology, biology, psychology,

and sociology, and frequently demonstrates his thesis in dramatic hyperbole. Over thirty years ago Giddings warned "against optimistic confidence that the long struggle for liberty of teaching was then practically ended." He saw "an increasing control of education by unenlightened political masses, carrying with it a submission of questions about the validity and worth of knowledge to a majority vote under universal suffrage" and he expected "that this subserviency of the high to the low will increase as the years go by." More recently scientific medicine is availing itself of chemistry and biology, and demonstrating the possibility of ridding the world of epidemic and other diseases; "but we scorn its teachings to follow after ignoramuses and quacks." "We have churches and schools . . . young men's and young women's Christian associations, colleges and universities; but we remain superstitious, afraid of knowledge, bigoted and intolerant, truculent, gun-toting and murderous." The supreme function of education in human society—"to apply intelligence and knowledge to the improvement of life"—has become more and more inhibited.

Education is the victim of the conflict between two irreconcilable conceptions of it: the sacrosanct and the experimental. It suffers from a persistent occultism that makes "Sunday more sacred than the personal example of the Lord, the flag more sacred than the country that flies it, and a written constitution more sacred than the rights and liberties it defines and guarantees." The virus of its disease is The Mighty Medicine, made in tribal antiquity by the medicine man, and perpetuated in increasingly virulent forms by practitioners of pow wow to the present day. The remedy is The New Liberal Education, with its mystery-dispelling knowledge, its rational goals, and its scientific methods.

The author sets up attainable goals, not neglecting the realm of values, and proposes means for their attainment, appropriate, respectively, to the lower schools and college, and to dull and bright or oblique and sterling pupils. He frequently draws heavily upon psychology, both directly and indirectly, and, in the opinion of the reviewer, with good judgment. Psychologists will find interest in this book.

Stanford University J. E. Coover

Strategy in Handling People. By E. T. Webb and John J. B. Morgan. Chicago, Boulton Pierce & Company, 1930. Pp. 260.

This book presents earmarks of strategy in its presentation as well as in its contents. Under the caption "Thanks are due to these scientists," the authors explain the "unusual plan of presenting this material," and "acknowledge a debt of gratitude to the following members of the American Psychological Association who gave timely encouragement in the early stages of this work by expressing their approval of the method used." Then follow the names (each with title or degree—Professor, Dean, or Dr.) of 75 members of the American Psychological Association. "The men and women listed above," we read in a note below the list, "are among the 159 scientists who approved of the basic plan of this book: to illustrate psychological principles with incidents from the careers of successful men. Their approval was given to the authors in response to a letter asking their estimate of various methods of presentation. There was, in some cases, a difference of opinion as to what additional methods should be used, but all agreed on the essential point." Psychologists of the Association will recall the

letter in question. Was their 'approval' an indication that they would approve whatever the authors chose to call "psychological principles," and whatever illustrations were chosen? Nothing in the statements of the book indicates that it was not. Who were the other 84 approving scientists, and how many or what percentage of the members of the American Psychological Association did not respond favorably to a plan which the note of dedication shows had been in preparation "over a period of several years" during which bibliographical data had been gathered and sifted? Those members, who failed to reply or who openly expressed doubts as to the validity of such a compilation, are not referred to at all.

The book presents in a popular style, enlivened with photographs of and quotations from leading executives in American life, the principles of strategy reported to have been utilized by those who have become successful. While such selected quotations and opinions are, of course, not to be confounded with psychology, or with any science, the general principles, 'secrets,' etc. (usually printed in italics) deduced by the authors are probably in the main useful to readers. An example of such principles is this: "When you want people to adopt one of your ideas and act on it with whole-hearted interest, it is often wise to let them think it is their own, and to give them credit for originating it" (p. 47). The reviewer has tried the book on some young men and has found that it has considerable interest-value. There is much repetition of materials on similar principles. It would be instructive for some one to attempt a determination of the reliability and validity of the dicta of the book. J.P.

Character through Creative Experience. By WILLIAM C. BOWER. Chicago, University of Chicago Press, 1930. Pp. 276.

This book represents an effort on the part of the author to use a more or less psychological technique for character education. It is interesting to the psychologist as illustrating how one who evidently is familiar with modern psychology would go about using this knowledge for the desired end: character through creative experience. Religion he considers an intrinsic desire for an end. There is some confusion, however, when he designates it as a "pull from in front," and tries to differentiate this type of impulsion from *vis a tergo*. There is a happy discussion of such topics as: integration and personality, motives in learning, and behavior patterns. Under proper guidance learning becomes to the growing personality a creative experience. The author states that one of the primary functions of religion is integration of personality. Though this, in the opinion of the reviewer, is a statement one would desire to make, there may be some doubt about its being more than felicitous theorizing as yet.

University of Denver T. R. GARTH

The Pattern of Life. By A. ADLER. Ed. by W. B. Wolfe. New York, Cosmopolitan Book Corporation, 1930. Pp. 273.

The 'pattern' is exemplified by a dozen case-studies of children displaying defects in conduct. By means of these Adler exhibits his method of studying the individual history, conversing with parents and child, drawing conclusions, and giving advice. The method shows common sense and an apprehension of the state and needs of badly adjusted children. It involves fewer Latin labels for diseases,

hypothetical interpretations, and cultish peculiarities than we have come to expect from those medical people who indulge in the speculative excesses of psychoanalysts and some others. The editor of the volume has prefaced the cases with a plain statement of Adler's theory of conduct, which is ambiguously and pretentiously called *Individual Psychology*. The "great principles" of the theory are set down as the unitary personality, the dynamic whole, the life pattern, and the individual strife for a goal under an apprehension of weakness and limitation. These principles strike the reviewer as more or less common property of all those who sensibly regard the conduct of youthful deviates. As Adler recedes from Freud, he seems to reduce his dependence upon such bold and speculative concepts as made impressive the teachings of his uncompromising master.

In his introductory essay the editor well says that "Individual Psychology is an art more than a science" (p. 43); but no one can read through Adler's sensible comments upon the problems of childhood without observing that, quite apart from its claim to be a 'psychology,' the method may be, in the hands of a wise and informed person, a useful way of helping children toward a more healthful development and parents toward a less stupid conception of their young.

M. B.

The Psychology of the Common Branches. By WILLIAM HENRY PYLE. Baltimore, Warwick & York, 1930. Pp. vii, 381.

The author has limited himself largely to four school subjects,—reading, spelling, handwriting, and arithmetic. Incorporated with the usual data are more recent experiments and opinions relating to measurements in these subjects and the methods and hygiene of instruction. Every chapter is followed by exercises, numbered statements of all the essential principles, brief abstracts of articles and books in the field, and blank pages for entering of references and abstracts of articles still unpublished.

Howard University F. C. SUMNER

An Introduction to Human Problems. By HAROLD BENJAMIN. Cambridge, Houghton Mifflin Co., 1930. Pp. xiii, 472.

This book's value to psychology lies almost exclusively in the field of methodology: approach, description, and analysis. The first four chapters—I. Introducing the Problem Solver; II. The Search for Order; III. The Description of Order; and IV. Science as Generalization—merit the perusal of all elementary students in psychology if not of all psychologists. Chapter III merits especial attention, not because of newness but because of its clarity and completeness. One sees in these early sections the influences of Kelley, the author's former teacher. Other sections impress one more with the breadth of their erudition and charm of style—often almost pedantic—than with depth of psychological understanding. The simplicity and finality of this treatment leave one with a feeling that the writer is unaware of works like Clark's *The New Evolution*. Intended as an orientation for college freshmen the book begins magnificently but presents such contradictions as the discerning and aggressive freshman will resent.

Michigan Central State Teachers College MILTON B. JENSEN

The Child's Emotions. Proceedings of the Mid-West Conference on Character Development, February, 1930. Chicago, University of Chicago Press, 1930. Pp. x, 406.

This symposium, comprising the contributions of seven psychologists, four physiologists, four physicians, three sociologists, two authors and lecturers, two school teachers, a public school superintendent, a religionist, a philosopher, and a university president, "dared to be more scientific and less general than ever before" (p. 5). Despite this temerity, the treatment was obviously and frankly popular, including, as antipodes apparently, "both the theories of psychologists, and the experience of those actually engaged in research work with children" (p.5). Very few factual or numerical data are given; most of the contributions being interpretative. Such unsubstantiated statements as, "There were slightly higher scores for the women than for the men," "there was little correlation," "the emotionally maladjusted do better than," etc., are frequent, and they make tiresome reading. The limits of childhood are stretched considerably, some of the contributions dealing with the babe at birth, and others with college seniors. There was little agreement as to the nature of emotion.

The book contains nothing that is new, unless it be a renewed emphasis upon psychoanalytic interpretations, there being no less than twenty references to the work of Freud and his followers.

DePauw University STEPHEN M. COREY

Psychology, Normal and Abnormal. By. J. W. BRIDGES. D. Appleton & Co., 1930. Pp. xxii, 552.

This book of twenty-nine chapters applies the fairly unique principles first proposed by Störring and now well-grounded in psychiatrical practice of making abnormal conditions the outgrowth or exaggeration of normal conditions. It proceeds by defining in rather simple terms the subject of psychology together with some of the main points of view. The reviewer does not agree, however, that "structuralism is thus a static point of view" or that "functional psychology is more concerned with the sequence of mental events than with their analyses." "Mental processes" rather than "mental states" found indeed a more worthy exponent in Titchener's structural psychology than with functional psychology. The date of the foundation of the first laboratory at Leipzig is given as 1870, which must be a misprint. The relation of psychology to other sciences, including the pseudo-sciences, is rather competently given in limpid language. The subject of the abnormal is introduced through a discussion of the norm as statistically derived. After discussing other points of view with regard to mind and body, the author swings over to the double aspect theory which is today finding more and more adherence. Then follows a discussion of the neural mechanisms of behavior including such well-known concepts as the neurograms and the more recent attitude with regard to the integrating alliances in the localization of function.

The author pays his respects to the unconscious in a very conservative fashion and then returns to the usual sectional headings of many of our text-books in psychology. In this case, however, he describes briefly first the normal situation and then under appropriate paragraph headings the abnormal conditions of sensitivity, affective response, reflex connections, and instinct through the ac-

quisition of skilled movements and linguistic habits. This is followed by a treatment of the higher mental processes of memory, imagination, thought, and belief.

With the eighteenth chapter, he turns to more intimately related psychopathic situations with an introductory review of suggestion, hypnosis, play, work, and fatigue. The subject of mental conflict follows that of motivation which leads us into the various interpretations of sleep and dreams where considerable space is given to the Freudian theory.

At the end we are led through chapters on temperament, character, personality and similar larger integrations and disintegrations of the mental life. Finally there is a summary of psychopathology with a succinct description of the main psychoses together with their subdivisions. The book ends with a summary of underlying theories in applied psychology, especially in the realm of psychotherapeutic methods. A comprehensive bibliography topically arranged is appended.

Although a number of books in this general field have lately been issued, it is the reviewer's impression that no other book has been so well systematized as this one. There is always a great temptation to press into service a long list of case studies. To a certain extent such studies furnish interesting and valuable material but the present book reduces this approach to a vanishing minimum and thus furnishes a fairly new experiment in treatises of this type.

University of Iowa CHRISTIAN A. RUCKMICK

Experiments and Exercises in Educational Psychology. By DANIEL STARCH. 3rd ed., by the author and Harry F. Latshaw. New York, The Macmillan Company, 1930. Pp. ix, 254.

Chapters are added on grade placement, personality analysis, study methods, methods of evaluating text-books, and intelligence and achievement testing. Certain chapters are considerably revised and two chapters are omitted. The added exercises are rather elementary and practical in nature. The experiments on certain phases of learning contain some good material, but many problems are omitted entirely.

Vanderbilt University LYLE H. LANIER

The Character Value of the Old Testament. By GEORGE W. BEISWANGER. University of Iowa Studies: Studies in Character, Vol. 3, No. 3. Iowa City, University of Iowa Studies, 1st Ser., no. 187, 1930. Pp. 63.

For generations the literature of the Bible has been regarded as "the chief staple of moral and religious instruction." The author questions the soundness of this evaluation and has sought to determine just what value for character building the Old Testament stories do possess. The techniques and procedures for rating and assigning school grades to 63 Old Testament stories and 65 entire books retelling such stories were slightly modified from those used by the staff of the Institute of Character Research in judging children's literature. The same staff of seven, who had worked together in judging fairy tales, assigned the ratings and grades. The conclusions reached indicate that nine-tenths of these Old Testament stories and three-fourths of the books of retold stories are not worth the child's attention, that, in any case, only three of the Old Testament stories can be recommended for use below the sixth school grade and that there are few good Old Testament story-books below that grade.

In evaluating the ethical content of the stories, restrictions were set up which seem to the reviewer to discount too completely the setting of the story, its traditional accretions and its historical perspective. It is hard to escape the impression that although the ratings and grades are made with meticulous care, they are the rather unconvincing pronouncements of a small staff of workers.

Lehigh University — JAMES L. GRAHAM

The Growth of Ability. By RAYMOND O. FILTER and OMAR C. HELD. Baltimore, Warwick & York, 1930. Pp. vii, 174.

Ability is defined as a demonstrated achievement; capacity, as a surmised quality, possibly the foundation of ability. Spearman's theory of the nature of ability is dismissed, not so much on the basis of the defects of the theory as on account of his misapplication of the term ability.

Conditioning comprises the major portion of perceptual learning. An excellent theoretical account of the growth of perception is given. The process of learning is described completely in terms of conditioning. Redintegration is viewed as but a specialization of the principle of conditioning. Movement-produced stimuli (including both exteroceptive and proprioceptive) are given a justly prominent position in the formation of serial responses. Such responses are, however, shown to be far from 'automatic.' The account of the elimination of errors includes no extra principle beyond that of acquiring new behavior. A 'wrong' response is automatically eliminated by the fixing of an antagonistic one. A relatively large section of the book is given to application and other practical considerations.

One of the chief virtues of the book is the fact that it gives a consistent account of the process of learning without recourse to the invention of new theories. The reviewer has seen no more successful effort to reduce the many facts of learning to a simple system. This does not imply, however, that the discussion is reduced to a too elementary level.

Duke University — HOWARD EASLEY

The Child's Conception of Physical Causality. By JEAN PIAGET. Tr. by Marjorie Gabain. New York, Harcourt, Brace & Co., 1930. Pp. viii, 309.

The French edition of which this book is a translation has already been reviewed in the JOURNAL (41, 1929, 481-483). The translator has rendered a valuable service to English readers who do not know French. The clearness and simplicity of the original version has been matched in the translation, but, unfortunately, the rather free translation occasionally does not give the exact meaning of the original work, especially where technical terms are employed. For instance, *concrète* is translated as "practical" (p. 1); *l'esprit parcourt un certain nombre de stades* is rendered "the human *spirit* goes through a certain number of stages" (p. 237, italics mine). Here *mind* would certainly have been better for English readers. With this important work by Piaget now more generally available to American readers, it is to be hoped that his experiment will be repeated in different quarters with better technique (including the use of adults as controls) than the author has employed.

J. P.

The Measurement of Man. By J. A. HARRIS, C. M. JACKSON, D. G. PATERSON, and R. E. SCAMMON. Foreword by C. H. Bailey. Minneapolis, The University of Minnesota Press, 1930. Pp. vii, 215.

This book is a compilation of four Sigma Xi lectures delivered by the authors at the University of Minnesota. It is semi-popular in nature and appears to have been designed for the educated layman. Part I, by Harris, deals with The Measurement of Man in the Mass; Parts II, by Jackson, with Normal and Abnormal Human Types; Part III, by Paterson, with Personality and Physique; and Part IV, by Scammon, with The Measurement of the Body in Childhood. Paterson made a thorough though brief review of the materials presented in greater detail in his book *Physique and Intellect.* Scammon presented, from the endocrinological point of view, some significant curves.

Peabody College PAUL L. BOYNTON

The Revolt Against Dualism: An Inquiry Concerning the Existence of Ideas. By ARTHUR O. LOVEJOY. New York, W. M. Norton & Co., Inc., 1930. Pp. xii, 325.

The author defends the hypothesis of representative ideas, which is today probably no less popular among philosophers than among American psychologists. While it is fortunate to have so able a representative of dualism the psychologist can only wish that the treatment of dualism versus some sort of monism could have been more directly and clearly drawn, and that the real issues could have been evaluated by means of more definite criteria. Few experimental psychologists will have the patience to go through this big volume for the various qualitive estimates, pro and con, of questions discussed from the standpoint of adults after years of habituation to certain views and terms and without resort to scientific methods and to genetic approaches. J. P.

Am Ende der Philosophie: Ein kritisches Wort an die Zeit und ihre Philosophen. By LUDWIG FREUND. Munich, Ernst Reinhardt, 1930. Pp. 178.

This is a critical examination into the limitations of philosophy. The author foresees the termination of any kind of philosophy which does not see its own limitations. On the basis of what he regards as rigid logical procedures he concludes that apodictic modality judgments are out of place in philosophy as well as in the empirical sciences. There is, of course, nothing new in such conclusions.

J. P.

Voices from Beyond. By HENRY HARDWICKE. Niagara Falls, Harkell Co., 1930. Pp. 125.

Although this book purports "to put into readable form certain well authenticated facts concerning psychical phenomena" established by persons "whose personal integrity it is impossible to question," there is little convincing evidence of 'mediums' and their work. The book cannot be recommended as anything like a scientific treatise. The author has presented his material in the form of a story, consequently the critical reader is unable to separate fact from fiction.

Peabody College WILSON L. NEWMAN

Philosophy of Value: An Essay in Constructive Criticism. By LEO R. WARD. New York, The Macmillan Co., 1930. Pp. x, 263.

This is a philosophical (or probably better theological) discussion from the scholastic point of view of the illusive problem of 'values.' The author has attempted a critical appraisal of a wide range of modern writers from the standpoint of Thomas Aquinas' *Summa Theologiae.* The book is of little interest to the modern psychologist, except, possibly, to illustrate the influence of a person's cultural setting and social contacts on his (intended) rationalistic position. The work is heavily documented with references and there is a bibliography of 26 pages.

J. P.

Discovering Ourselves: A View of the Human Mind and How It Works. By EDWARD A. STRECKER and KENNETH E. APPEL. New York, The Macmillan Co., 1931. Pp. xiii, 306.

The authors say: "This book is meant to be entirely practical. It is not written for psychologists or psychiatrists." The book presents the usual Freudian concepts in a popular non-critical manner.

Illinois Military School J. D. DODSON

Die Religion in ihrer dogmatischen und ihrer reinen Form: Versuch einer Grundlegung der Religionsphilosophie. By LEO HAMBURGER. Munich, Ernst Reinhardt, 1930. Pp. 170.

This book is not concerned with religious behavior as actually found in man, nor with individual differences in this regard, but is theoretical and somewhat absolutistic. It fails to adhere to present psychological conceptions of determination and will, and it does not employ scientific methods.

J. P.

Solution of the World-Riddle. Book One: Epistemology, the Natural Science of the Universe. By INNOMINATUS. San Francisco, Harr Wagner Publishing Co., 1931. Pp. xxxii, 197.

As indicated in the title and anonymous authorship, this book has no scientific value for the psychologist. It can be recommended only as a curious example of disordered elaboration.

Cornell University CHARLES WARREN FOX

Le génie des religions. Second Edition. By BARON DESCAMPS. Paris, Félix Alcan, 1930. Pp. xxii, 539.

Nothing of importance to psychology has been added in this edition. The book is not written from a scientific point of view. There is no excuse for the size of this volume: its contents could be put into relatively few pages if stated in a direct, matter-of-fact style. Its verbosity impresses one as a compensation for lack of real content.

J. P.

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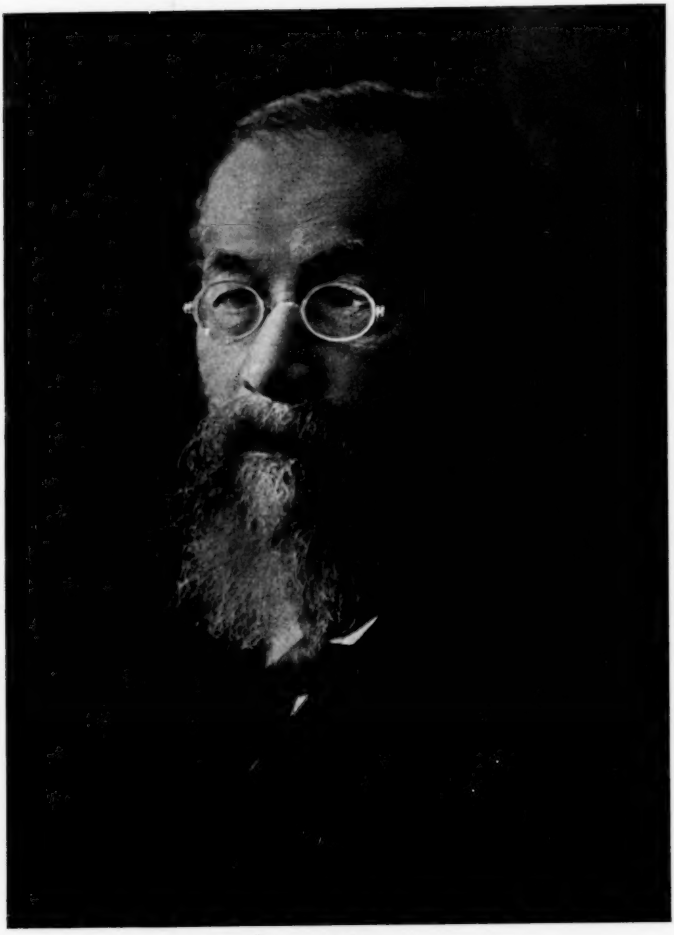

H. Schmidt.